Healthcare financing systems for increasing the use of tobacco dependence treatment

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TABLE OF CONTENTS

[Intervention Review]

Healthcare financing systems for increasing the use of tobacco dependence treatment

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ABSTRACT

Background

Tobacco smoking is the leading preventable cause of death worldwide, which makes it essential to stimulate smoking cessation. The financial cost of smoking cessation treatment can act as a barrier to those seeking support. We hypothesised that provision of financial assistance for people trying to quit smoking, or reimbursement of their care providers, could lead to an increased rate of successful quit attempts. This is an update of the original 2005 review.

Objectives

The primary objective of this review was to assess the impact of reducing the costs for tobacco smokers or healthcare providers for using or providing smoking cessation treatment through healthcare financing interventions on abstinence from smoking. The secondary objectives were to examine the effects of different levels of financial support on the use or prescription of smoking cessation treatment, or both, and on the number of smokers making a quit attempt (quitting smoking for at least 24 hours). We also assessed the cost effectiveness of different financial interventions, and analysed the costs per additional quitter, or per quality-adjusted life year (QALY) gained.

Search methods

We searched the Cochrane Tobacco Addiction Group Specialised Register in September 2016.

Selection criteria

We considered randomised controlled trials (RCTs), controlled trials and interrupted time series studies involving financial benefit interventions to smokers or their healthcare providers, or both.

Data collection and analysis

Two reviewers independently extracted data and assessed the quality of the included studies. We calculated risk ratios (RR) for individual studies on an intention-to-treat basis and performed meta-analysis using a random-effects model.

Main results

In the current update, we have added six new relevant studies, resulting in a total of 17 studies included in this review involving financial interventions directed at smokers or healthcare providers, or both.

Full financial interventions directed at smokers had a favourable effect on abstinence at six months or longer when compared to no intervention (RR 1.77, 95% CI 1.37 to 2.28, I^2 = 33%, 9333 participants). There was no evidence that full coverage interventions increased smoking abstinence compared to partial coverage interventions (RR 1.02, 95% CI 0.71 to 1.48, I^2 = 64%, 5914 participants), but partial coverage interventions were more effective in increasing abstinence than no intervention (RR 1.27 95% CI 1.02 to 1.59, I^2 = 21%, 7108 participants). The economic evaluation showed costs per additional quitter ranging from USD 97 to USD 7646 for the comparison of full coverage with partial or no coverage.

There was no clear evidence of an effect on smoking cessation when we pooled two trials of financial incentives directed at healthcare providers (RR 1.16, CI 0.98 to 1.37, I^2 = 0%, 2311 participants).

Full financial interventions increased the number of participants making a quit attempt when compared to no interventions (RR 1.11, 95% CI 1.04 to 1.17, I^2 = 15%, 9065 participants). There was insufficient evidence to show whether partial financial interventions increased quit attempts compared to no interventions (RR 1.13, 95% CI 0.98 to 1.31, I^2 = 88%, 6944 participants).

Full financial interventions increased the use of smoking cessation treatment compared to no interventions with regard to various pharmacological and behavioural treatments: nicotine replacement therapy (NRT): RR 1.79, 95% CI 1.54 to 2.09, I^2 = 35%, 9455 participants; bupropion: RR 3.22, 95% CI 1.41 to 7.34, I^2 = 71%, 6321 participants; behavioural therapy: RR 1.77, 95% CI 1.19 to 2.65, I^2 = 75%, 9215 participants.

There was evidence that partial coverage compared to no coverage reported a small positive effect on the use of bupropion (RR 1.15, 95% CI 1.03 to 1.29, I^2 = 0%, 6765 participants). Interventions directed at healthcare providers increased the use of behavioural therapy (RR 1.69, 95% CI 1.01 to 2.86, I^2 = 85%, 25820 participants), but not the use of NRT and/or bupropion (RR 0.94, 95% CI 0.76 to 1.18, I^2 = 6%, 2311 participants).

We assessed the quality of the evidence for the main outcome, abstinence from smoking, as moderate. In most studies participants were not blinded to the different study arms and researchers were not blinded to the allocated interventions. Furthermore, there was not always sufficient information on attrition rates. We detected some imprecision but we judged this to be of minor consequence on the outcomes of this study.

Authors' conclusions

Full financial interventions directed at smokers when compared to no financial interventions increase the proportion of smokers who attempt to quit, use smoking cessation treatments, and succeed in quitting. There was no clear and consistent evidence of an effect on smoking cessation from financial incentives directed at healthcare providers. We are only moderately confident in the effect estimate because there was some risk of bias due to a lack of blinding in participants and researchers, and insufficient information on attrition rates.

PLAIN LANGUAGE SUMMARY

Do interventions that reduce the cost of smoking cessation treatment increase quit rates, quit attempts or use of treatments?

Background

Interventions that reduce or cover the costs of smoking cessation medication and behavioural support could help smokers quit. We reviewed the evidence about the effects of financial interventions directed at smokers and healthcare providers on medication use, quit attempts and successful quitting.

Study characteristics

We searched all relevant studies that involved financial interventions directed at smokers and healthcare providers. For smokers, the aim of the healthcare financing interventions had to be to encourage the use of smoking cessation treatment or making successful quit attempts. For interventions directed at healthcare providers, the intervention had to stimulate the healthcare provider to assist people with quitting smoking, for example by prescribing smoking cessation treatment.

Key results

For the update of this review, we searched studies on the effect of financial interventions on smoking cessation treatment and success in September 2016. We found six new relevant studies, resulting in a total of 17 studies.

We found 15 studies directed at smokers. Covering all the costs of smoking cessation treatment for smokers (free treatment) when compared to providing no financial benefits increased the number of smokers who attempted to quit (4 studies, 9065 participants), used smoking cessation treatments (7 studies, 9455 participants), and succeeded in quitting (6 studies, 9333 participants).

We found three studies directed at healthcare providers. The two studies that investigated the effect of a financial intervention on quit success (2311 participants) did not clearly show an increase in quit rates. Financial interventions directed at healthcare providers also did not have an effect on the use of smoking cessation medication (2 studies, 2311 participants). However, financial interventions did increase the number of smokers who used smoking cessation counselling (3 studies, 25,820 participants).

Information on the costs of the intervention was available for eight studies (33,488 participants). The economic evaluation of the individual studies showed that although the absolute differences in quitting were small, the costs per person successfully quitting were low or moderate.

Quality of the evidence

We concluded that financial interventions directed at smokers increase the proportion of smokers who attempt to quit, use smoking cessation treatments, and succeed in quitting. We did not detect a clear effect on smoking cessation from financial incentives directed at healthcare providers. This review has some limitations that affect how confident we can be in the conclusions. The included studies varied substantially in quality and in methods and design, which makes it difficult to compare results.

Summary of findings for the main comparison. Interventions directed at individuals: full financial coverage compared to no financial coverage for increasing abstinence from smoking

Interventions directed at individuals: full financial coverage compared to no financial coverage for increasing abstinence from smoking

Patient or population: smokers
Setting: medical practises, companies, members of a health insurance company, outpatient respirology clinic
Intervention: full financial coverage for abstinence from smoking
Comparison: no coverage

Outcomes	Anticipated absolute effects* (95% CI)		Relative effect (95% CI)	№ of participants (studies)	Quality of the evidence (GRADE)	Comments
	Risk with no coverage	Risk with full financial coverage				
Abstinence from smoking	Study population		RR 1.77 (1.37 to 2.28)	9333 (6 RCTs)	⊕⊕⊕⊖ Moderate[1,2]	
	84 per 1000	149 per 1000 (115 to 192)				

*__The risk in the intervention group__ (and its 95% confidence interval) is based on the assumed risk in the comparison group and the __relative effect__ of the intervention (and its 95% CI).

CI: Confidence interval; **RR:** Risk ratio

GRADE Working Group grades of evidence
High quality: we are very confident that the true effect lies close to that of the estimate of the effect
Moderate quality: we are moderately confident in the effect estimate: the true effect is likely to be close to the estimate of the effect, but there is a possibility that it is substantially different
Low quality: our confidence in the effect estimate is limited: the true effect may be substantially different from the estimate of the effect
Very low quality: we have very little confidence in the effect estimate: the true effect is likely to be substantially different from the estimate of effect

[1]Downgraded one level because of risk of bias: all studies except for Hughes 1991 had a serious risk of bias.
[2]We rated Hughes 1991 and Twardella 2007 serious for imprecision, and Pakhale 2015 very serious for imprecision, however, we did not downgrade the evidence because these studies were small and had a minor effect on the outcome.

Summary of findings 2. Interventions directed at healthcare providers compared to placebo for increasing the use of smoking cessation treatment

Interventions directed at healthcare providers compared to no interventions for increasing the use of smoking cessation treatment

Patient or population: physicians and clinics from a multispecialty group practice
Setting: health clinics in the USA and group practices in Germany
Intervention: financial interventions directed at healthcare providers (pay for performance and direct payment)
Comparison: no financial intervention

Outcomes	Anticipated absolute effects* (95% CI)		Relative effect (95% CI)	№ of participants (studies)	Quality of the evidence (GRADE)	Comments
	Risk with no interventions	Risk with interventions directed at healthcare providers				
Abstinence from smoking	Study population		RR 1.16 (0.98 to 1.37)	2311 (2 RCTs)	⊕⊕⊕⊖ Moderate[1]	
	181 per 1000	209 per 1000 (177 to 247)				

***The risk in the intervention group** (and its 95% confidence interval) is based on the assumed risk in the comparison group and the **relative effect** of the intervention (and its 95% CI).

CI: Confidence interval; **RR:** Risk ratio

GRADE Working Group grades of evidence
High quality: we are very confident that the true effect lies close to that of the estimate of the effect
Moderate quality: we are moderately confident in the effect estimate: the true effect is likely to be close to the estimate of the effect, but there is a possibility that it is substantially different
Low quality: our confidence in the effect estimate is limited: the true effect may be substantially different from the estimate of the effect
Very low quality: we have very little confidence in the effect estimate: the true effect is likely to be substantially different from the estimate of effect

[1]Downgraded one level because both studies were judged to be at serious risk of bias.

BACKGROUND

Tobacco smoking is a major risk factor for non-communicable diseases. A range of interventions such as pharmacotherapy and counselling are currently in use to treat nicotine dependence (Aveyard 2007). Although the majority of smokers attempt to quit unassisted (Edwards 2014), treatments are recommended, as they can increase quit success (West 2000; Aveyard 2007; Nides 2008; Lancaster 2017; Stead 2017). Existing management strategies for smoking cessation remain limited in effectiveness since a significant majority of smokers attempting to quit either fail to quit or relapse after a successful quit (Foulds 2006; Mitrouska 2007; West 2015). Apart from counselling and pharmacotherapy, strategies such as financial interventions may be useful to address the problem in a multifaceted manner and increase cessation rates (Coleman 2001; Benowitz 2008).

Regardless of scientific acceptability, treatment utilisation by smokers and selection of treatment strategies by healthcare providers and insurance companies are influenced by cost considerations, as documented in non-experimental studies (Cox 1990; Gencarelli 2003; Briesacher 2007; Goto 2007; Hollenbeck 2011). As a result, costs and a variety of financial interventions are becoming increasingly important considerations in medical practice to improve patient outcomes and quality of service (Cheung 1997; Peckham 2010; Van Herck 2010). For instance, an international survey reported that in several countries a larger proportion of physicians are likely to receive financial incentives based on quality improvement targets: the percentage of physicians provided with incentives was 89% in the UK; 81% in the Netherlands; 70% in Italy; and 65% in Austria (Schoen 2009).

Despite their popularity, financial interventions are shown to have at best weak or mixed impacts on service quality and patient outcomes (Steel 2007; Peckham 2010; De Bruin 2011; Flodgren 2011; Ryan 2016). In a review of 128 studies that implemented financial incentives in healthcare, Van Herck and colleagues indicated that, for incentive schemes to be effective, they need to be applied to individuals or teams and have prior set targets for quality and patient outcome improvements (Van Herck 2010). Reviews also indicate that there is little or no information on the cost-effectiveness of these incentives (Van Herck 2010; De Bruin 2011; Scott 2011).

According to reports, smoking cessation treatments are highly cost effective compared to other, commonly implemented preventive interventions, such as hypertension and cholesterol-lowering treatments (Cheung 1997; Parrot 2004). In a systematic review of the literature, Ronckers and colleagues reported that in spite of lack of standardised reporting by the included studies, smoking cessation treatments aimed at reducing smoking prevalence are cost-effective (Ronckers 2005).

In light of limited resources and the increasing cost of health care, it is pertinent to examine the impact of financial interventions provided to smokers and healthcare providers on treatment and process-related outcomes such as smoking cessation service use. We hypothesised that provision of financial assistance for smokers trying to quit, or reimbursement of their healthcare providers, could lead to an increased rate of quit attempts, utilisation of smoking cessation treatments, and successful quitting. This review evaluates financial interventions directed at individual smokers regardless of whether they achieve cessation and also evaluates financial interventions directed at healthcare providers. A separate Cochrane Review evaluates competitions and incentives to reward smokers who achieve cessation or abstinence in smoking cessation schemes (Cahill 2015).

OBJECTIVES

The primary objective of this review was to assess the impact of reducing the costs of providing or using smoking cessation treatment by healthcare financing interventions on abstinence from smoking. The secondary objectives were to examine the effects of different levels of financial support on the use or prescription of smoking cessation treatment, or both, and on the number of smokers making a quit attempt (quitting smoking for at least 24 hours). We also assessed the cost effectiveness of different financial interventions, and analysed the costs per additional quitter, or per quality-adjusted life year (QALY) gained.

METHODS

Criteria for considering studies for this review

Types of studies

We considered randomised controlled trials (RCTs), controlled trials (CTs) and interrupted time series (ITS) studies. We included ITS studies if they had at least three time points before and three after the intervention, and if they had a clearly defined intervention point.

Types of participants

We included those studies in which the study population consisted of smokers or healthcare providers. We assessed the primary and secondary objectives from either a smoker's or a healthcare provider's perspective. For smokers, the aim of the healthcare financing interventions had to be to encourage the use of smoking cessation treatment and successful quit attempts. When the intervention was directed towards healthcare providers, the intervention had to aim to affect the prescribing of smoking cessation treatment or the smoking behaviour of the participants, or both, by offering assistance to quit smoking.

Types of interventions

We included trials that studied the effects of healthcare financing interventions directed at smokers or healthcare providers for increasing the use of smoking cessation treatment (e.g. delivered by government, healthcare insurance plans, or other institution-arranged interventions).

We classified patient-centred financial interventions as:

- Health insurance coverage - changes to the level of benefit available for smoking cessation treatments, including changes to copayment or out-of-pocket payments made by people receiving treatment.
- Direct coverage - changes to the direct cost to the smoker of using smoking cessation treatment, for example by provision of a prescription for free pharmacotherapy.
- Health insurance cost - changes to the premiums or user fees paid for health insurance.

We defined healthcare provider-centred financing interventions as:

- Salary - payment for a set number of working hours or sessions per time unit.
- Capitation - a set amount of payment per patient for providing specific care.
- Fee-for-service - payment for every item of service or unit of care provided.
- Target payment (pay for performance, P4P) - payment only made with respect to achievement of an agreed target.
- Fund holding- and organisation-level payment systems - which can improve the working conditions within an organisation, and can indirectly influence the salary of a healthcare provider.

We differentiated between healthcare financing interventions for individual smokers and for healthcare providers. In patients, for example, comparisons can be made between full financial coverage and partial financial coverage. For healthcare providers, a maximum target payment can be compared with no target payment. There were no restrictions on the type of smoking cessation treatment for which the financial benefit could be offered. This could include pharmacotherapy (e.g. nicotine replacement therapy, varenicline or bupropion), behavioural support, or a combination thereof. When the financial intervention of a study was aimed at more than one type of smoking cessation treatment, the effect of the financial intervention could be spread out over the different types of products. Studies of financial interventions that are aimed at more than one type of smoking cessation treatment therefore cannot formally be compared with studies that offer coverage for only one product. This is the reason for differentiating between partial and full interventions as discussed in the next paragraph. As a result we focused on full and partial financial interventions. As a smoker could use more than one type of product, summing the use of the different types of smoking cessation treatment could overestimate the number of smokers who used smoking cessation treatment.

We classified the patient-directed financial benefits as full, partial or no intervention, based on theoretical and practical considerations. We considered an intervention that covered the cost of both pharmacotherapy and behavioural support to be a full financial intervention. Full financing need not come from the trial. If there were already existing provisions (e.g. insurance) for partial coverage of smoking cessation treatment (pharmacotherapy or behavioural support) and a trial's intervention(s) complemented this benefit by financing either a pharmacotherapy or behavioural support leading to full coverage of smoking cessation treatment, we classified it as a full-coverage intervention. In this case we considered the control group to be partial financial intervention. More specifically, if an intervention provided coverage for either pharmacotherapy or behavioural support, we considered it partial financial coverage. If a study involved gradations of partial financial intervention, we considered the one with more benefits from the trial's perspective to be the intervention group while the group receiving lesser benefits was the control group. If, in a given study, a control group received neither trial-based nor already existing financing arrangements for smoking cessation treatment, we considered it a no-financing intervention. On these bases, we employed further stratification depending on the availability of study data.

Types of outcome measures

We included studies when they used at least one of the following outcome measures to describe the effects of the intervention. The primary outcome measure of this review was:

- abstinence from smoking. We included studies reporting abstinence from smoking at least six months after the start of the intervention, and we used the longest available follow-up as the preferred outcome measure (SRNT 2002; Hughes 2003). Biochemically validated abstinence was preferred to self-reported abstinence, and continuous or prolonged abstinence was preferred to point-prevalence abstinence.

The secondary outcome measures were:

- number of participants making a quit attempt, defined as the number of participants who attempted to quit at least once. A quit attempt is defined as not having smoked for at least 24 hours.
- Use of smoking cessation treatment, defined as the number of participants who reported having used smoking cessation treatment or who were registered by healthcare providers or medical insurance organisations as having used smoking cessation treatment.

Economic Evaluation

To evaluate the cost effectiveness of financial interventions for smoking cessation treatment, we considered data from studies that examined both cost and effects and compared two or more alternatives. The primary outcome measure of the economic evaluation was smoking-related:

- costs per additional quitter.

The secondary outcome measure was:

- costs per quality-adjusted life year saved (QALY). This measure of health outcome incorporates the effect of an intervention on both the length and the quality of life.

Search methods for identification of studies

We identified eligible studies for the current update by electronic search of the Cochrane Tobacco Addiction Group Specialised Register, limited to records added since April 2012. This Register includes citations identified via highly sensitive searches for potential reports of controlled trials and other evaluations of interventions for smoking cessation and prevention. At the time of the search on 1 September 2016, the Register included the results of searches of the Cochrane Central Register of Controlled trials (CENTRAL; 2016, issue 7) in the Cochrane Library; MEDLINE (via OVID), to update 20160729, Embase (via OVID), to update week 201639 and PsycINFO (via OVID) to update 20160725. See the Tobacco Addiction Group Module in the Cochrane Library for full search strategies and list of other resources searched. The search strategy (Appendix 1) included MeSH and text terms related to health care costs, health insurance coverage, reimbursement, remuneration, incentives, salaries and fees.

There was no limitation on language. The Cochrane Tobacco Addition Group's Information Specialist performed the register search, and also prescreened retrieved records for relevance to the topic.

In order to retrieve unpublished studies, we contacted experts in the field via a standardised email. We included unpublished studies or abstracts only when sufficient data were available.

For the original review and first update, additional searches of MEDLINE (Appendix 2) and Embase were conducted that combined topic-related terms with the same smoking- and design-related terms used in the regular searches for the Register. Since these records are a subset of the records retrieved and screened during regular searches we judged this no longer necessary.

The Cochrane Tobacco Addiction Group's glossary of smoking-related terms can be found in Appendix 3.

Data collection and analysis

Selection of studies

Based on title, keywords and abstract, three reviewers (FB, GN, AAR) each selected studies independently of each other by applying the inclusion criteria to the studies identified by the literature search. When there was any doubt whether to select a study or not, we resolved it through discussion. Two reviewers (FB and GN) assessed the full-paper versions of the selected studies in duplicate for each study and independently of each other. If disagreements about inclusion were not resolved by consensus, we arranged to consult additional reviewers (DK and AAR).

Data extraction and management

Two reviewers (FB and GN) separately and independently extracted data from the included studies, after which the extracted data were compared and discussed. Any disagreements between the two reviewers were resolved by discussion. We extracted the following data.

- Methods: setting (location of care, country, and year of study) and study design
- Participants and/or healthcare providers: method of recruitment, inclusion criteria, characteristics of study population (smoking status, age, gender and motivation to quit smoking)
- Interventions: description of the intervention for each group
- Outcome measures: definition for each study of continuous abstinence or point-prevalence abstinence, number of participants making a quit attempt, prescription and use of smoking cessation treatment
- Results: we extracted the findings of each study for pooling

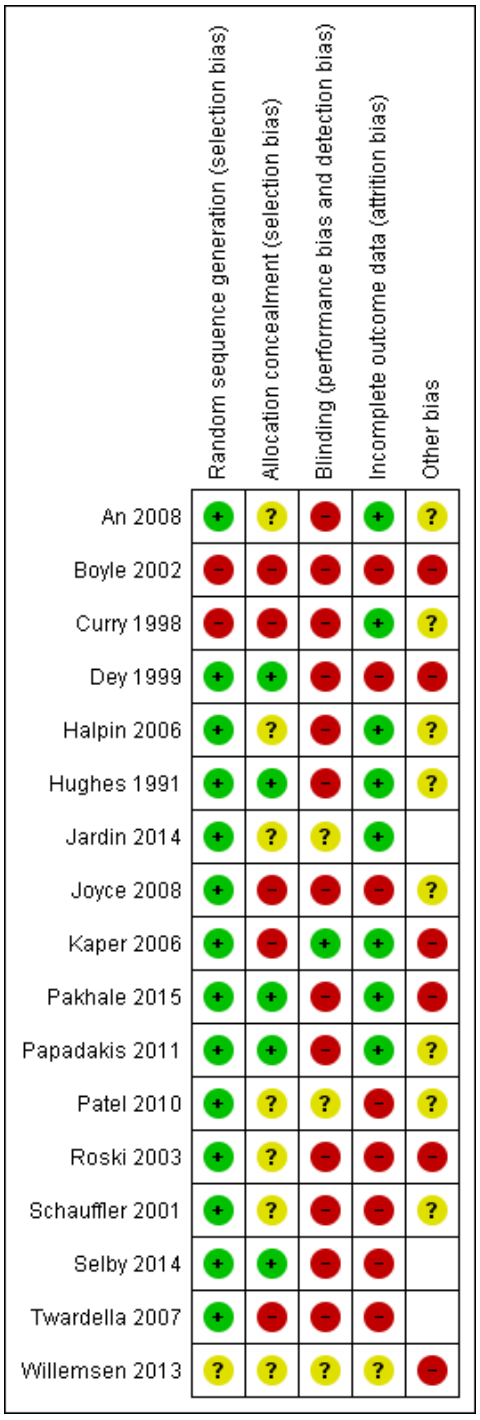
Two reviewers (AAR and SE) extracted data concerning the economic evaluations that we used to answer our secondary objectives. We resolved any discrepancies by discussion. We extracted the following data.

- Perspective and time horizon of the economic evaluation
- Direct costs: volume and value of costs of the use of smoking cessation treatment, costs of consultations with healthcare providers and overhead costs (no research costs)
- Indirect costs: volume and value of general medical care, lost productivity, time and travel costs spent by participants visiting healthcare providers
- Discounting and sensitivity analyses
- Results of the economic evaluations

Assessment of risk of bias in included studies

We assessed the risk of bias of the included studies using criteria from Cochrane included in the Review Manager (RevMan) 5 software (Higgins 2011; RevMan 2014). The criteria have three levels (low risk of bias, high risk of bias, unclear risk of bias) and the judgement on each item could be complemented using quotes from the study report and the raters' comments. We rated the following criteria: sequence generation; allocation concealment; blinding of intervention; issues of incomplete data and loss to follow-up; and other unclassified bias identified by the review authors. We summarised findings from these criteria in Figure 1.

Figure 1. Risk of bias summary: review authors' judgements about each risk of bias item for each included study



The reviewers assessing risk of bias were not blinded to the authors, institution or journal title. Two reviewers (FB and GN) independently rated the studies. We held a consensus meeting to discuss and resolve disagreements between the two reviewers. If a study did not contain sufficient information on methodological criteria or the information was unclear, we contacted the study authors for additional information.

Economic Evaluation

We assessed the quality of the economic evaluations using the Consensus Health Economic Criteria (CHEC) list (Evers 2005). As its name indicates, the CHEC list was produced based on expert consensus. It consists of 19 items as listed in Appendix 4 and incorporates the following aspects: clearly described study population (age, gender and educational level), a description of the intervention and the alternatives, a well-defined research question, an economic study design in which the costs and effects

of two or more interventions are compared, a time horizon and perspective of the evaluation, the identification of relevant costs and consequences for each alternative, the measurement of costs and consequences, appropriately valued cost and consequences, the performance of an incremental analysis, the performance of discounting and sensitivity analysis, the conclusions following from the data reported, the generalisability of results, statement of conflict of interest and appropriate discussion of ethical and distributional issues. SE independently assessed the quality of the economic evaluations. Items scored as 'yes' received one point. Items scored as 'unclear' or 'no' received no points. We calculated a total score by summing the score of the 19 items (range 0 to 19).

Measures of treatment effect

We used only intention-to-treat analyses. If the study did not present an intention-to-treat analysis then we recalculated the published data on an intention-to-treat basis. We counted all dropouts and participants lost to follow-up as continuing smokers or making no quit attempt or not having used smoking cessation treatment. For each study outcome we calculated the risk ratio (RR) and the corresponding 95% confidence interval (CI) as a measure of intervention effect. Since the outcomes (abstinence, quit attempts and use of treatment) were favourable, a ratio greater than unity indicates an outcome favouring financial intervention. Where we calculated pooled effects, we used a Mantel-Haenszel method to estimate the RR using a random-effects model. We considered pooling when at least two trials assessed the effects of healthcare financing interventions and reported data on the same outcome measure. We conducted a formal statistical test for between-studies variance, and assessed whether the observed variability in effect sizes was greater than would be expected to occur by chance (sampling error). We used the I^2 statistic, given by the formula ((Q-df)/Q)*100%, to investigate heterogeneity (where Q represents Cochrane's Chi^2 statistic and df is the degree of freedom, Higgins 2003). We considered a pooled analysis to have moderate and high heterogeneity when the I^2 statistic was more than 50% and 75% respectively.

Economic Evaluation

The transferability of cost estimates of different economic evaluations is mostly restricted by differences in setting. These differences can be related to patient characteristics, incidence of smoking-related diseases, availability of health resources, variations in clinical practice, incentives to healthcare providers and relative prices or costs (Drummond 2015). Pooling of the different economic evaluations is only permissible when there is no interaction between the setting and the effect of the intervention on medical consumption (Drummond 2015). Where pooling was allowed, the volumes of medical consumption, like the use of smoking cessation treatment, were to be pooled and multiplied with the pooled costs per unit consumption. The total costs were calculated in US dollars (USD). When the cost estimates of the different economic evaluations were not transferable, we presented cost data of the individual studies. When no incremental ratios were presented, we calculated the incremental cost effectiveness ratios ourselves. First, we calculated the total costs per group. We then divided the difference in costs between the groups by the difference in number of quitters between the groups. The authors of the studies involved checked the calculation. When currencies other than USD were involved, we used the exchange rates provided by study authors.

'Summary of findings' table

Following standard Cochrane methodology (Schünemann 2011), we created 'Summary of findings' tables for our primary outcome for interventions directed at smokers (Summary of findings for the main comparison) and interventions directed at healthcare providers (Summary of findings 2). This included a GRADE evaluation of the quality of evidence, based on the five standard considerations (study limitations, consistency of effect, imprecision, indirectness and publication bias) (Guyatt 2011).

RESULTS

Description of studies

Results of the search

We identified 3518 references using the search strategy described above, of which 28 were identified from the Register search for this update (for a PRISMA flow chart see Figure 2) (Moher 2009). After we had removed duplicates, 23 studies remained, of which six new studies were included in the current update (Patel 2010; Papadakis 2011; Willemsen 2013, Jardin 2014; Selby 2014; Pakhale 2015) and 17 were listed as excluded, with reasons (see Excluded studies table). With the six new studies added in this update, we included a total of 17 studies in this review.

Figure 2. Prisma study flow diagram of studies included in most recent update of this review

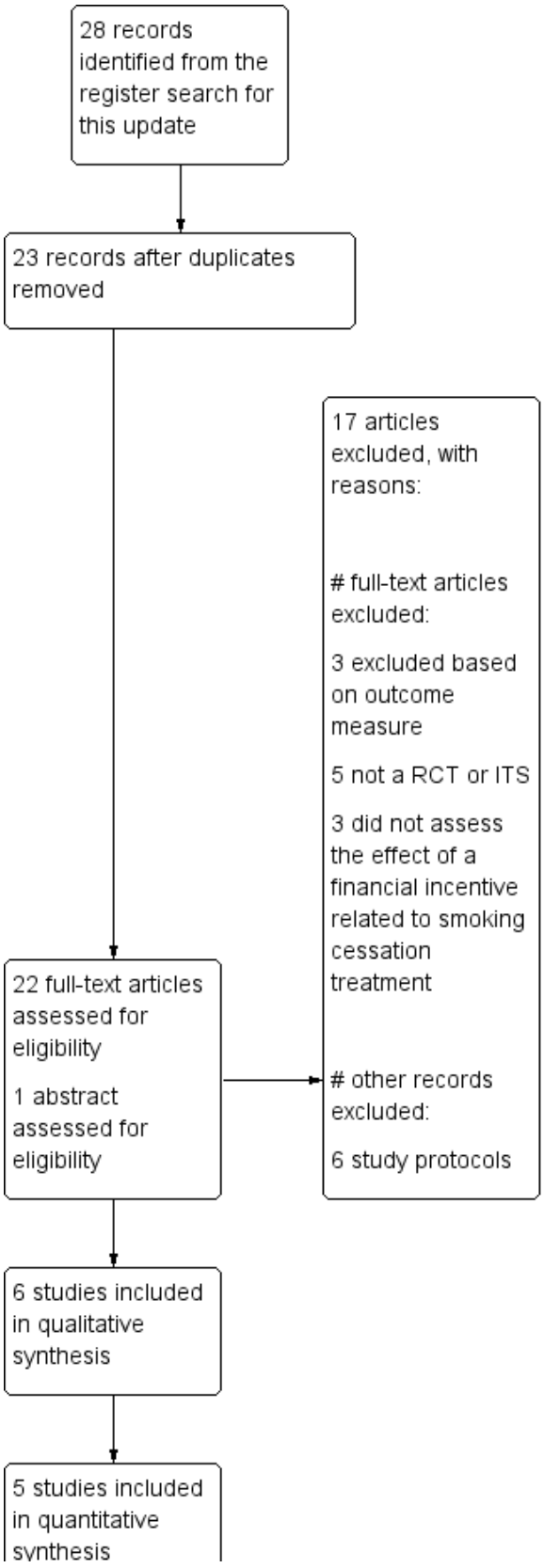
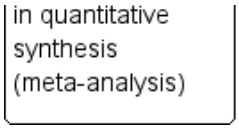


Figure 2. (Continued)



Included studies

Full details of the 17 included studies are given in the Characteristics of included studies table; we describe the main features below.

Setting and design

Ten of the included studies were performed in the USA (Hughes 1991; Curry 1998; Schauffler 2001; Boyle 2002; Roski 2003; Halpin 2006; An 2008; Joyce 2008; Patel 2010; Jardin 2014). The others were conducted in Canada (Papadakis 2011; Selby 2014; Pakhale 2015), the UK (Dey 1999), the Netherlands (Kaper 2006; Willemsen 2013) and Germany (Twardella 2007). Four studies were conducted in co-operation with health insurance organisations (Curry 1998; Schauffler 2001; Boyle 2002; Kaper 2006). Two studies were conducted in family practices (Hughes 1991; Dey 1999), two in an Ottawa hospital clinic (stroke prevention: Papadakis 2011, respirology: Pakhale 2015) and one study was conducted in 40 clinics of a multi-speciality medical group practice (Roski 2003). Of the seventeen included studies, eleven randomly assigned the individual participants to the treatment group and one or two control groups (Hughes 1991; Dey 1999; Schauffler 2001; Roski 2003; Halpin 2006; Kaper 2006; Patel 2010; Papadakis 2011; Jardin 2014; Selby 2014; Pakhale 2015) and three randomly assigned medical practices (Twardella 2007; An 2008; Joyce 2008). Two studies (Curry 1998; Boyle 2002) were controlled natural experiments with two and four different benefit groups, respectively. One study used a descriptive time-series analysis (Willemsen 2013).

Participants

Fifteen studies were directed at individuals (Hughes 1991; Curry 1998; Dey 1999; Schauffler 2001; Boyle 2002; Halpin 2006; Kaper 2006; Twardella 2007; Joyce 2008; Patel 2010; Papadakis 2011; Jardin 2014; Selby 2014; Willemsen 2013; Pakhale 2015). Sample sizes of the included studies varied from 28 participants in Papadakis 2011 to 7354 in Joyce 2008. All participants were at least 18 years old. The age of the participants in the included studies varied from a mean of 38 to more than 65 years. Six studies included a general population of smokers (Curry 1998; Schauffler 2001; Halpin 2006; Kaper 2006; Patel 2010; Willemsen 2013. Dey 1999 and Selby 2014 included only smokers who were motivated to quit. Half of the sample included in Boyle 2002 was motivated to quit smoking. All of the participants in An 2008; Papadakis 2011 and Pakhale 2015 were motivated to quit smoking in the next 30 days. Participants in Hughes 1991 did not have to be motivated to quit to participate in the study, but were allowed to withdraw from the study after they were told that they would be randomly assigned to different price groups. Participants were voluntarily enrolled and motivated to quit in Joyce 2008. Jardin 2014 included smokers not wanting to quit in the next 30 days.

Three studies assessed the effects of financial interventions directed at healthcare providers (Roski 2003; Twardella 2007; An 2008). Twardella 2007 involved both patient- and healthcare provider-directed interventions. In the studies that involved healthcare provider-directed interventions, patient behaviour was measured using a baseline survey and a follow-up survey after six months.

Interventions

Patient-directed interventions

Six studies investigated the effect of changes to the level of insurance coverage for smoking cessation treatment (Curry 1998; Schauffler 2001; Boyle 2002; Kaper 2006; Willemsen 2013; Selby 2014). Nine studies investigated the effect of changes to the direct cost to the smoker of receiving treatment (Hughes 1991; Dey 1999; Halpin 2006; Twardella 2007; Joyce 2008; Patel 2010; Papadakis 2011; Jardin 2014; Pakhale 2015). Coverage was offered for four different types of smoking cessation treatment: nicotine replacement therapy (NRT), bupropion, varenicline and behavioural support. Three studies each covered two types of smoking cessation treatment: Boyle 2002 offered coverage for NRT (patches and gum) and bupropion, and Curry 1998 and Schauffler 2001 covered NRT (patches and gum) and participation in a behavioural programme. Kaper 2006; Halpin 2006; Twardella 2007 and Joyce 2008 covered three types of therapy: NRT (patches, gum, sublingual tablets and lozenges), bupropion and behavioural support. Papadakis 2011 and Selby 2014 covered NRT, bupropion and also varenicline. In Pakhale 2015 participants were offered free telephone counselling and they received a USD 110 voucher for pharmacotherapy of their own choice. In Willemsen 2013 smokers could be reimbursed by their insurance company for pharmacotherapy if they also engaged in (free) telephone counselling. The treatment periods ranged from two weeks (Jardin 2014) to one year (Curry 1998; Schauffler 2001; Boyle 2002; Kaper 2006 Joyce 2008 and Selby 2014). The included studies also varied in the extent of insurance coverage or treatment cost and the comparisons made. Seven studies compared full coverage of the cost of treatment with no coverage (Hughes 1991; Schauffler 2001; Kaper 2006; Twardella 2007; Joyce 2008; Willemsen 2013; Pakhale 2015). Five studies reported the effect of partial interventions as compared to no financial intervention (Hughes 1991; Boyle 2002; Papadakis 2011; Jardin 2014; Selby 2014). One study compared full coverage of both behavioural treatment and NRT with a partial benefit requiring a 50% co-payment for either behavioural or NRT components (Curry 1998) and two studies investigated the differences between a cost to the patient of USD 20, USD 6/USD 10 or USD 0 per box of nicotine gum (Hughes 1991; Patel 2010). Joyce 2008 provided four categories of benefit ranging from usual care to benefits of pharmacotherapy and counselling.

Healthcare provider-directed interventions

Roski 2003 distributed printed versions of smoking cessation guidelines to clinics in both the intervention and control group. The intervention group clinics were eligible for payments for reaching targets for registration of participants' smoking status and providing advice to quit. Twardella 2007 provided a tutorial to general practitioners on how to conduct counselling and

prescribe pharmacotherapy to help smokers; additionally, the practitioners were provided with a financial remuneration of EUR 130 for each participant they treated and who was biochemically confirmed to have quit at the end of six months' follow-up. An 2008 compared usual care and a pay-for-performance intervention offering USD 5000 for 50 quitline referrals. Pay-for-performance clinics also received monthly updates on their referral numbers.

Outcomes

Abstinence from smoking after six months or more from the start of the intervention was the primary and preferred outcome. Boyle 2002 presented self-reported continuous (more than six months) abstinence rates. The longest follow-up report comes from Kaper 2006 in which biochemically validated continuous abstinence was reported at 12 months, while Selby 2014 reported biochemically validated continuous abstinence for weeks 26-52 and Twardella 2007 and Papadakis 2011 reported biochemically validated continuous abstinence at six months' follow-up. Seven studies presented self-reported point prevalence abstinence data (Hughes 1991; Curry 1998; Schauffler 2001; Roski 2003; Halpin 2006; Joyce 2008; Pakhale 2015). Three studies reported biochemically validated point prevalence abstinence data (Patel 2010; Papadakis 2011; Selby 2014). In Hughes 1991, observers named by the participants were asked to verify reported smoking status of participants. Dey 1999 assessed abstinence from smoking at 14 weeks after the start of the reimbursement period and Jardin 2014 at 12 weeks; therefore, we did not include data from these studies in the analysis of the effects of reimbursement on abstinence rates. Pakhale 2015 had problems with data collection and therefore measured point prevalence abstinence at different endpoints between 26 and 52 weeks, with an average of 33 weeks after baseline.

One of the secondary outcome measures was the number of participants who made a quit attempt. Eight studies presented data on this outcome (Hughes 1991; Schauffler 2001; Boyle 2002; Halpin 2006; Kaper 2006; Papadakis 2011; Jardin 2014; Selby 2014). The other secondary outcome measure was the self-reported use or registered use of smoking cessation treatment. This was self-reported in Schauffler 2001; Boyle 2002; Kaper 2006; Papadakis 2011; Jardin 2014 and Pakhale 2015, registered by a health maintenance organisation in Curry 1998, and by the local pharmacy in Hughes 1991; Dey 1999; Halpin 2006; An 2008 and Selby 2014. Utilisation of smoking cessation services was reported by general practitioners in Twardella 2007 and by telephone counsellors in Papadakis 2011. Roski 2003 recorded self-reports by smokers regarding the use of bupropion or NRT and/or any counselling services. Willemsen 2013 used data on treatment enrolment recorded by the national quit line. Patel 2010 only registered the number of nicotine boxes acquired by participants and not actual medication use.

Eight of the 17 included studies presented data on the costs of the intervention, and compared the costs and effects of the intervention with one or two alternatives (Hughes 1991; Curry 1998; Schauffler 2001; Halpin 2006; Kaper 2006; Twardella 2007; An 2008; Joyce 2008). Most of the studies used a time horizon equal to the duration of the intervention, and all used a third party payer perspective, in which only the direct costs of the intervention were presented. Curry 1998 also presented a user's perspective. The cost effectiveness ratio was presented in terms of costs per user who quit smoking or costs per participant enrolled. No study presented data in terms of quality-adjusted life years (QALY) saved. For Kaper 2006, additional information on outcomes on quit attempts, use of treatment and cost were collected from two other related reports (Kaper 2005a, Kaper 2006a).

Excluded studies

Of the 60 studies for which we assessed full reports for eligibility, we excluded 45 (see Characteristics of excluded studies). Six studies were research protocols without data (Courtney 2014; Ostroff 2014; Harter 2015; Bonevski 2016; Park 2016; NCT00962988). Although NCT00962988 and Ostroff 2014 had not yet been published at the time of writing this review, we assessed them as probably relevant and therefore registered as ongoing studies. The Characteristics of excluded studies table summarises the reasons for exclusion. Most excluded studies were not randomised controlled trials, controlled trials or interrupted time series (ITS) studies (Alberg 2004; Cummings 2006). Oswald 1988 and Cox 1990 retrospectively compared the outcomes of using free and purchased gum in a non-randomised trial. Russos 1999; Parnes 2002 and Stone 2002 all used a cross-sectional design. Bailey 2016 was a cohort study and Hamilton 2013 a systematic review. Fiore 2000, Coleman 2001, Doescher 2002, Latts 2002, Ringen 2002, Solberg 2002, Amundson 2003, Chang 2008 and Weisman 2012 did not have a control group and also did not use an ITS design. Land 2010 used an ITS design but did not meet the required inclusion criterion of three measuring points before and three after the intervention for the outcomes quit attempts and quit success and we therefore excluded it. There were other reasons for exclusion in addition to study design. Lave 1996 compared two different financial systems in two different settings, and did not report data on the smoking status of the control group. The financial intervention in nine studies (Curry 1991; Hovell 1996; Russos 1999; Donatelle 2000; Pardell 2003; Volpp 2006; Kruse 2013; McLeod 2015; Moskowitz 2016) was not directly related to the use of smoking cessation treatment. Krist 2010 was excluded since its intervention of free counselling was not directed specifically at smoking but also at 'unhealthy' behaviours such as drinking alcohol. Hays 1999 and Hockenberry 2012 were excluded because they did not explicitly and directly assess the effects of a financial intervention. Shaw 2003 assessed the effect of nicotine gum prices on the use of gum and abstinence rates, and did not report the number of participants using smoking cessation treatment or the quit rate. We excluded Walsh 2012; Amemori 2013; Bardach 2013 and Verbiest 2013 because they did not measure relevant outcome measures for this review. Fu 2016 was aimed at assessing the effect of proactive counselling and lacked a suitable control group.

Risk of bias in included studies

The summary results of our methodological assessment are displayed in Figure 1.

Allocation

Ten studies stated that individual participants were randomly allocated to the different benefit groups (Hughes 1991; Dey 1999; Schauffler 2001; Halpin 2006; Kaper 2006; Patel 2010; Papadakis 2011; Jardin 2014; Selby 2014; Pakhale 2015). Three studies randomly allocated clinics to study conditions and then identified smokers (Roski 2003; Twardella 2007; An 2008). One study conducted randomisation based on the location or geography of the primary care clinics (Joyce 2008). Although the exact method for generating the randomisation sequence was generally not reported, we classified all these 14 studies as having a low risk of

bias for this part of the design. Three studies (Curry 1998; Boyle 2002 and Willemsen 2013) were not randomised trials and did not involve experimental manipulation of conditions. Willemsen 2013 used a natural on-off design in the entire Dutch population and we classified it as having an unclear risk of bias. Boyle 2002 and Curry 1998 made use of natural experiments in which different insured groups were receiving different smoking cessation benefits. This had the potential for bias due to differences between people in different benefit groups, and therefore we classified these two studies as a having a high risk of bias.

Five studies reported allocation concealment with enough detail to judge there to be a low risk of selection bias (Hughes 1991; Dey 1999; Papadakis 2011; Selby 2014; Pakhale 2015). Six studies did not give enough information on allocation concealment procedure and we therefore assessed them as having an unclear risk of bias (Schauffler 2001; Halpin 2006; Jardin 2014; Patel 2010; An 2008;). We judged five studies to be at high risk of bias (Curry 1998; Boyle 2002; Kaper 2006; Twardella 2007; Joyce 2008). For Curry 1998 and Boyle 2002 allocation concealment was not directly applicable because benefit groups were not experimentally manipulated, for the other three studies, we judged allocation concealment to be inadequate. For Willemsen 2013 allocation concealment was not applicable because of its ITS design.

Blinding

Kaper 2006 blinded the participants in the control group to the treatment available to the experimental group and evaluated the success of the blinding and concluded that it was successful. Therefore, this was the only study that we assessed as being at low risk of bias. We judged three studies to be at unclear risk of bias due to lack of information provided. We judged 13 studies to be at high risk of bias because of incomplete) or no blinding (An 2008; Boyle 2002; Curry 1998; Dey 1999; Halpin 2006; Hughes 1991; Joyce 2008; Pakhale 2015; Papadakis 2011; Roski 2003; Schauffler 2001; Selby 2014; Twardella 2007). In Papadakis 2011 clinicians and participants were not blinded, but the research co-ordinator conducting the outcome assessment was blind to group allocation. Selby 2014 unblinded the investigator and participant before deciding on the smoking cessation method to be used, but both were blinded to the results of cotinine tests.

Incomplete outcome data

We assessed eight studies to be at low risk of attrition bias (Hughes 1991; Curry 1998; Halpin 2006; Kaper 2006; An 2008; Papadakis 2011; Jardin 2014; Pakhale 2015); the other studies we assessed to be at high risk of attrition bias. In Boyle 2002 participants were self-selected respondents to a survey so an ITT analysis was not possible. In addition, the study authors did not account for 20% 'unusable sample' excluded from the analysis. Dey 1999 and Joyce 2008 had different attrition rates in the experimental and control groups, and did not provide an explanation for this difference, so it was unclear whether attrition was at random or not. Patel 2010 reported significant different attrition rates between experimental groups. In Roski 2003 it was not reported how non-response or loss to follow-up were handled in the analysis. Schauffler 2001 did not provide sufficient information on missing data and did not report an ITT. In Selby 2014 a higher proportion of the participants in the control group discontinued the study because they were 'no longer willing to participate'. The follow-up rate was less than 80% for all the included studies except in Curry 1998; Halpin 2006; Twardella 2007 and Jardin 2014, which had higher rates. In Patel 2010 55% of the participants in the control group compared to 13% in the intervention group withdrew from the study. Five studies did not address incomplete data (Dey 1999; Schauffler 2001; Boyle 2002; Roski 2003; Twardella 2007). Hughes 1991; Halpin 2006; Kaper 2006; Papadakis 2011; Jardin 2014 and Selby 2014 used an ITT analysis.

Other potential sources of bias

We classified six studies as being at high risk of another source of bias (Dey 1999; Boyle 2002; Roski 2003; Kaper 2006; Willemsen 2013; Pakhale 2015). In Boyle 2002; Kaper 2006 and Pakhale 2015 self-reports of quitting or abstinence were not properly validated. Dey 1999 lacked power and reported only short-term abstinence. Roski 2003 reported a possible Hawthorne effect on all clinics regardless of randomisation status. A potential source of bias for the ITS study of Willemsen 2013 is that the start of the reimbursement period was accompanied by a large media campaign to encourage smokers to apply for the free smoking cessation programme. We judged eight studies to be at unclear risk of bias (Hughes 1991; Curry 1998; Schauffler 2001; Halpin 2006; An 2008; Joyce 2008; Patel 2010; Papadakis 2011), for details see Characteristics of included studies). There was no or unclear baseline comparability reported with regard to important predictors such as smoking levels and dependence, age, sex, income etc. in four studies (Hughes 1991; Boyle 2002; Roski 2003; Joyce 2008). In Papadakis 2011 participants in the intervention group reported smoking more cigarettes per day and they also reported a higher self-efficacy with quitting than participants in the control group. Patel 2010 reported a higher mean number of cigarettes smoked per day in the intervention group and an unequal distribution of male/female participants across intervention groups.

Quality of economic evaluation studies

The methodological quality assessment regarding the economic evaluations is presented in Appendix 4. The score of the five studies varied between 6 and 15 (out of a possible maximum of 19). In only one study were all relevant costs identified, measured and valued appropriately (Kaper 2006). For example, costs of visits to healthcare providers were not measured, no contact times were presented, the volumes of the use of smoking cessation treatment were incomplete and the sources of cost valuation were not described. Incremental analyses and sensitivity analyses were not performed. Direct costs were not discounted, but this was appropriate as the time frame of the cost analysis was less than 12 months. No statements of potential conflicts of interest were presented.

Effects of interventions

See: **Summary of findings for the main comparison** Interventions directed at individuals: full financial coverage compared to no financial coverage for increasing abstinence from smoking; **Summary of findings 2** Interventions directed at healthcare providers compared to placebo for increasing the use of smoking cessation treatment

To determine the general effect of healthcare financing interventions, we performed meta-analysis using a random-effects model. When only one study examined the effects of an intervention on a specific outcome, we presented the results of this individual study graphically.

The effect of financial interventions directed at smokers

Abstinence from smoking

In this section we report the effect of financial interventions directed at smokers on continuous and point prevalence abstinence rates. The reported abstinence rates were stratified into four different intervention subgroups and the pooled effect of each of the financial benefit combinations is reported.

Full coverage versus no financial intervention

Six studies reported the effect of full versus no financial intervention. Among these, two reported biochemically validated continuous abstinence rates at six months (Twardella 2007) and one year (Kaper 2006). The other studies reported six-month (Hughes 1991; Schauffler 2001) and 12-month (Joyce 2008) self-reported point prevalence abstinence, and abstinence between 26 and 52 weeks (Pakhale 2015), which we considered as six-month abstinence. In all six studies the abstinence rate favoured the intervention group, however, only Schauffler 2001; Kaper 2006 and Joyce 2008 reached statistical significance. Upon pooling of the six studies there was a statistically significant favourable effect of financial interventions on abstinence but with a moderate level of heterogeneity (RR 1.77, 95% CI 1.37 to 2.28, I^2 = 33%, 9333 participants) (Analysis 1.1).

Full coverage versus partial coverage interventions

Five studies reported the effect of full compared to partial financial interventions. One study reported six-month CO-validated continuous abstinence (Papadakis 2011), the other studies reported six-month (Curry 1998; Hughes 1991), eight-month (Halpin 2006) and 12-month (Joyce 2008) self-reported point prevalence abstinence. Two of the studies, Hughes 1991 and Joyce 2008, had point estimates favouring the full intervention group but only Joyce 2008 reached statistical significance. Papadakis 2011 was underpowered to find statistically significant results. The overall pooled estimate did not show an additional positive effect of full interventions on top of partial interventions on smoking abstinence (RR 1.02, 95% CI 0.71 to 1.48, I^2 = 64%, 5914 participants) (Analysis 1.2).

Partial coverage versus another partial coverage intervention or no intervention

Five studies reported the effect of partial financial intervention as compared to no financial intervention on abstinence from smoking. Selby 2014 reported 12-month continuous abstinence and Boyle 2002 and Joyce 2008 reported 12-month point prevalence abstinence. Two studies reported six-month point prevalence abstinence: Hughes 1991 and Patel 2010 (CO-validated). The pooled estimate showed a favourable effect of financial intervention on abstinence rates (RR 1.27, 95% CI 1.02 to 1.59, I^2 = 21%, 7108 participants) (Analysis 1.3).

When assessing the effect of financial interventions involving one type of partial financial intervention compared with another, where the partial intervention group with more benefits was considered an experimental group, Curry 1998 was the only study that provided self-reported, six-month point prevalence abstinence. There was no statistically significant difference between the two intervention strategies on abstinence rates (RR 1.20, 95% CI 0.86 to 1.68, 298 participants) (Analysis 1.4).

Number of participants making a quit attempt

Four studies evaluated the impact of full financial interventions versus no intervention on quit attempts (Hughes 1991; Schauffler 2001; Kaper 2006; Joyce 2008). All four studies indicated a favourable effect of full financial intervention; this effect was statistically significant in Schauffler 2001 and Joyce 2008. When the estimates were pooled there was a small positive effect of full benefit on the rate of quit attempts with RR of 1.11 (95% CI 1.04 to 1.17, I^2 = 15%, 9065 participants) (Analysis 2.1).

Four studies assessed the impact of full versus partial financial interventions on the rate of quit attempts (Hughes 1991; Halpin 2006; Joyce 2008; Papadakis 2011). Joyce 2008 showed a small favourable effect of full financial incentives on quit attempts, but the pooled estimate did not show a statistically significant effect (RR 0.99, 95% CI 0.84 to 1.17, I^2 = 57%, 5486 participants) (Analysis 2.2).

Five studies reported the effect of partial financial interventions as compared to no financial benefit on the rate of quit attempts (Hughes 1991; Boyle 2002; Joyce 2008; Jardin 2014; Selby 2014). All studies had confidence intervals including the line of no effect except for Selby 2014, which showed a statistically significant beneficial effect of financial intervention. The pooled estimate marginally favoured partial financial interventions but was not statistically significant (RR 1.13, 95% CI 0.98 to 1.31, I^2 = 88%, 6944 participants) (Analysis 2.3). However, the studies showed substantial statistical heterogeneity.

Use of smoking cessation treatment

In this section we present the pooled estimates of financial interventions on the use of the smoking cessation treatments such as nicotine replacement therapy (NRT), bupropion, varenicline and behavioural interventions.

Full coverage versus no financial intervention

Seven studies reported outcomes on the number of smokers using smoking cessation treatment by study groups with full financial benefit compared to those with no benefit. The overall or pooled effect of full financial benefit compared to no benefit on the use of NRT, bupropion and behavioural interventions was positive and significant in each subgroup, though we could not pool the individual subgroups as four studies were included in multiple subgroups (Analysis 3.1).

The studies by Hughes 1991; Dey 1999; Schauffler 2001; Kaper 2006; Twardella 2007; Joyce 2008 and Pakhale 2015 reported the utilisation rate of NRT. In all studies full financial interventions increased the use of NRT except for Pakhale 2015, which showed a positive effect on NRT utilisation but did not reach statistical significance. The pooled estimate indicated a significantly higher use of NRT in participants receiving full interventions with a RR of 1.79 (95% CI 1.54 to 2.09, I^2 = 34%, 9455 participants) (Analysis 3.1).

Three studies reported the rate of utilisation of bupropion (Kaper 2006; Twardella 2007; Joyce 2008). All of the studies showed an increased use of bupropion in the financial intervention group. The pooled estimate also showed a large and significant positive effect of full financial intervention on the use of bupropion treatment with a RR of 3.22 (95% CI 1.41 to 7.34, I^2 = 71%, 6321 participants)

(Analysis 3.1). However, the high heterogeneity calls for a cautious interpretation.

Four studies reported on the utilisation of behavioural interventions (Schauffler 2001; Kaper 2006; Twardella 2007; Joyce 2008). Kaper 2006 and Joyce 2008 indicated a positive effect of full financial intervention on the use of behavioural smoking cessation therapy; Schauffler 2001 and Twardella 2007 also showed a positive effect on the use of therapy but the results were statistically non-significant. Upon pooling, there was substantial heterogeneity, but financial interventions had a statistically significant positive effect of on the use of behavioural therapy (RR 1.77, 95% CI 1.19 to 2.65, I^2 = 75%, 9215 participants) (Analysis 3.1). One study reported the combined use of NRT and oral medications (Pakhale 2015), which did not differ significantly between research groups that received a financial intervention and that did not (RR 1.11, 95% CI 0.73 to 1.68).

Full coverage versus partial coverage interventions

Six studies reported the impact of full compared to partial financial interventions on the utilisation of pharmacotherapy and behavioural support (Hughes 1991; Curry 1998; Halpin 2006; Joyce 2008; Papadakis 2011; Willemsen 2013). Because of its ITS design, we did not include Willemsen 2013 in the subgroup analysis. As Curry 1998 was included in multiple subgroups in this analysis, we were unable to pool the results and report usage of each type of therapy separately. One study showed a large and statistically significant positive effect of full interventions on the use of NRT (Curry 1998). Joyce 2008 was also significant, but the remaining two studies had similar sized smaller effects that were not statistically significant. There was a high level of heterogeneity in the estimate of the pooled effect of the four studies in regards to effect on use of NRT (RR 1.76, 95% CI 1.27 to 2.43, I^2 = 87%, 22,380 participants) (Analysis 3.2). Pertaining to use of behavioural interventions, there was a significantly increased use in Curry 1998 (RR 3.95, 95% CI 3.15 to 4.95, 16,922 participants) - the only study in this category. Willemsen 2013 measured the number of people using behavioural therapy via the national quitline before and after the introduction of a national reimbursement system. The number of participants increased from 848 smokers enrolled in the year before the reimbursement to 9091 smokers enrolled during the year after reimbursement was instated. When the reimbursement was discontinued, only 151 smokers enrolled in the first 18 weeks of that year. Both Halpin 2006 and Joyce 2008 reported the utilisation of bupropion, in which there was no statistically significant effect of full financial intervention as compared with partial financial intervention with a RR of 1.42 (95% CI 0.84 to 2.41, I^2 = 61%, 3700 participants) (Analysis 3.2) upon pooling. Papadakis 2011 had a very small sample size and showed no significant results on the use of pharmacotherapy in general (RR 1.19, 95% CI 0.70 to 2.02, 28 participants).

Partial coverage versus another partial coverage intervention or no financial intervention

Hughes 1991; Boyle 2002; Joyce 2008; Jardin 2014 and Selby 2014 reported on the effect of partial as compared to no financial intervention on utilisation of pharmacotherapy. The pooled estimate of the five studies showed a small positive but not statistically significant effect of the intervention on utilisation of NRT with a RR of 1.37 (95% CI 0.99 to 1.91, I^2 = 91%, 6944 participants) (Analysis 3.3). Unlike the other studies where participants had to take action themselves to receive pharmacotherapy, in Jardin 2014 participants in the intervention group were sent a two-week supply of NRT by mail, which could make the results not entirely comparable with the other studies. If Jardin 2014 is left out of the pooled results, the RR declines to 1.13 (95% CI 0.88 to 1.45, I^2 = 86%, 6840 participants) (Analysis 3.3). Boyle 2002; Joyce 2008 and Selby 2014 presented the effect of partial interventions on bupropion use. Members of the group with coverage for pharmacotherapy had a slightly higher use of bupropion than those without (RR 1.15, 95% CI 1.03 to 1.29, I^2 = 0%, 6765 participants) (Analysis 3.3).

A single study reported the effect of a partial financial intervention on the use of varenicline (Selby 2014). The analysis showed a positive result on the use of varenicline (RR 1.85, CI 1.68 to 2.03, 1380 participants) (Analysis 3.3). Only the small study of Jardin 2014 reported the use of behavioural interventions, which was not affected by the financial intervention (RR 0.77, 95% CI 0.22 to 2.71, 104 participants) (Analysis 3.3).

Curry 1998 was the only study that reported the impact of partial versus other partial financial interventions on the use of pharmacotherapy and behavioural support. It did not show an effect on the use of NRT or behavioural interventions (RR 0.83, 95% CI 0.68 to 1.02 and RR 0.82, 95% CI 0.61 to 1.11, 14,155 participants) (Analysis 3.4).

The effect of financial interventions directed at healthcare providers

Three studies reported the effect of financial interventions directed at healthcare providers (Roski 2003; Twardella 2007; An 2008). Roski 2003 and Twardella 2007 reported outcomes on abstinence. Neither showed statistically significant effects. When the effects of financial interventions on abstinence were pooled the results showed that interventions targeting healthcare providers did not affect abstinence from smoking (RR 1.16, 95% CI 0.98 to 1.37, I^2 = 0%, 2311 participants) (Analysis 4.1). In the same two studies, financial interventions did not influence the use of pharmacotherapy (NRT) when pooled, with a RR of 0.94 (95% CI 0.76 to 1.18, I^2 = 6%, 2311 participants) (Analysis 4.2). However, there was a statistically significant positive effect of interventions directed at healthcare providers on the use of behavioural support when the three studies (Roski 2003; Twardella 2007; and An 2008) were combined (RR 1.69, 95% CI 1.01 to 1.28, I^2 = 85%, 25,820 participants)(Analysis 4.3).

The cost effectiveness of financial interventions

Data on the costs of the intervention were available for eight studies (Hughes 1991; Curry 1998; Schauffler 2001; Halpin 2006; Kaper 2006; Twardella 2007; An 2008; Joyce 2008). All studies had interventions directed at smokers and Twardella 2007 also included an intervention directed at physicians. As pooling of the different economic evaluations is only allowed when there is no interaction between the setting and the effect of the intervention (Drummond 2015), we did not pool the results of the individual studies. For the most part, the review authors presented smoking-related outcomes and performed the incremental analyses . One study calculated the costs per QALY saved (Kaper 2006).

Hughes 1991 included the following direct costs: nicotine gum, a smoking cessation booklet and healthcare providers' time. Participants' time was regarded as an indirect cost. The calculated

financial gain per participant enrolled was USD 1120 with full coverage when gum was provided free, USD 280 when gum was provided at a cost of USD 6 per box and USD 413 when gum cost USD 20. For the incremental analyses, we calculated the costs per additional quitter for the different comparisons. When we compared a full incentive with a partial incentive, the costs per additional quitter were USD 260. When we compared a full incentive with no incentive, the costs were USD 716. A partial incentive was not cost effective when compared with no incentive.

Curry 1998 presented the direct costs of NRT and a behavioural intervention programme for the different coverage groups. Indirect costs were not registered. With full coverage, the average costs per benefit user who quit were USD 21 for users and USD 1117 for the health plan. With partial coverage, the costs per benefit user who quit were respectively USD 326 and USD 801. We also calculated the incremental cost-effectiveness ratio: if full coverage were introduced instead of partial coverage, the financial gain for users would be USD 5316 for each benefit user who quit. For the health plan, the costs would be USD 7646 per benefit user who quit.

Schauffler 2001 reported the total costs of NRT, the behavioural programme and the self-help kit for the treatment group, but not for the control group. The study authors have subsequently advised us that the control group costs amounted to USD 29 per participant, for the self-help kit. The average costs per quitter were USD 1495. The costs per additional quitter for full coverage compared with no coverage were USD 1247.

Halpin 2006 calculated the cost of treatment (self-help kit, NRT and proactive telephone counselling). The cost per prevalent abstinence at eight months were USD 449 per quitter in the 'drugs only' group and USD 842 in the 'drugs and counselling' group. The incremental cost-effectiveness ratio we calculated was USD 731 for each additional quitter in the 'drugs and counselling' group.

Kaper 2006 reported the costs of treatment per participant as USD 378 and USD 491 in the control group and intervention group, respectively. The costs per additional quitter were calculated by bootstrap replicated with a mean cost-effectiveness ratio of USD 1453 per quitter. The mean costs per additional QALY were USD 2342. If society is willing to pay USD 12,990 for an additional 12-month quitter, the probability that reimbursement for smoking cessation treatment would be cost effective was 95%.

Twardella 2007 (as reported in Salize 2009) indicated that the costs of treating a participant ranged from USD 0 in the treatment-as-usual group to USD 2039 in the training and incentive group, USD 6404 in the training and medication group and USD 12,821 in the training, incentive and medication group. We used the bootstrap replication method to calculate the costs per additional quitter, which were USD 108 and USD 97 in the two interventions compared to treatment as usual.

An 2008 indicated that the marginal cost for the intervention clinics was USD 83 per additional referral and USD 300 per additional enrollee for treatment.

In Joyce 2008, the additional cost per quitter in the intervention groups relative to the usual-care group ranged from USD 463 to USD 6450 per quitter. The costs escalated with increased use of resources for treatment.

DISCUSSION

We identified 14 RCTs, two controlled natural experiments and an ITS study to answer questions on the importance of financial interventions directed at smokers and healthcare providers in increasing abstinence from smoking, quit attempts by smokers, and the use of smoking cessation treatment, and where possible to assess their cost effectiveness. We investigated the effect of financial interventions by identifying comparison subcategories between full, partial, and no financial intervention. Full financial benefits directed at smokers provided very positive outcomes compared to no benefits on continuous abstinence, point prevalence abstinence, and utilisation of smoking cessation treatment. When compared to no benefits, full financial coverage showed a modest but positive effect on quit attempts. Though not consistently, full financial benefits seem to also have more beneficial effect than partial coverage. We detected a mixed effect of the different levels of financial interventions with regard to the different endpoints with low to high levels of between-studies variance (heterogeneity). There is scant evidence to pass judgement on financial interventions directed at healthcare providers, but the available evidence shows only limited impact on smoking cessation.

Summary of main results

We detected a statistically significant, positive effect of full financial interventions targeting smokers with regard to abstinence from smoking compared to provision of no financial intervention at six months' follow-up or more (all abstinence measures: RR 1.77, 95% CI 1.37 to 2.28, I^2 = 33%, 9333 participants). The effect of full financial interventions was also extended to favourable outcomes on the use of smoking cessation treatments: the pooled effect of full coverage compared with no financial intervention on the use of smoking cessation treatments was highly significant for each treatment type (NRT, bupropion, and behavioural interventions). Full financial interventions had a small, significant effect on quit attempts (RR 1.11, CI 1.04 to 1.17, I^2= 15%, 9065 participants).

When full coverage was compared to partial coverage, results showed no significant additional effect of full coverage on smoking cessation or quit attempts. Pooling together results from four trials, full coverage did increase the use of NRT when compared with partial coverage, though the level of heterogeneity was high (I^2 = 87%). There were not enough data to investigate the effects of full coverage versus partial coverage on the utilisation of bupropion or behavioural therapy. These findings could mean that full financial interventions may have a significant effect over partial interventions regarding the use of treatment. However, these findings should be interpreted with caution given the high heterogeneity, which reflects the reality that partial interventions could come in various shapes and combinations.

Upon pooling, we found a small positive effect of partial financial interventions compared to no interventions on abstinence (RR 1.27, CI 1.02 to 1.59, I^2 = 21%, 7108 participants). We did not detect an effect of partial financial interventions compared to other partial or no financial interventions on attempts at quitting. There may be an effect of partial financial interventions when compared to no interventions with regard to the use of pharmacotherapy. Upon the pooling of three studies, the use of bupropion was increased (RR 1.15, 95% CI 1.03 to 1.29, I^2 = 0%, 6765 participants), The use of NRT

was also increased but pooling of the studies just failed to reach statistical significance (RR 1.37, 95% CI 0.99 to 1.91, I^2 = 91%, 6944 participants). The single study that investigated varenicline (Selby 2014) showed a positive effect on its use (RR 1.85, 95% CI 1.68 to 2.03, 1380 participants).

Only three included studies were directed at healthcare providers, which should be considered when interpreting the results. We detected no effect of healthcare provider-directed benefits on continuous abstinence, point prevalence abstinence or on the use of NRT. However, healthcare provider-directed interventions compared to no intervention increased the use of behavioural therapy (RR 1.69, 95% CI 1.01 to 2.86, I^2 = 85%, 25,820 participants). Financial benefits may induce healthcare providers to provide behavioural-intervention support to smokers, though the high level of heterogeneity suggests results should be interpreted carefully. It should also be borne in mind that financial resource should not be invested merely for increasing smoking cessation treatments unless it can be translated into favourable quitting rates. Generally speaking, our findings seem to support the general trend of mixed and non-conclusive findings on healthcare provider-directed financial interventions for improving patient outcomes (Steel 2007; Peckham 2010; Van Herck 2010; De Bruin 2011; Flodgren 2011; Ryan 2016).

Eight studies (Hughes 1991; Curry 1998; Schauffler 2001; Halpin 2006; Kaper 2006; Twardella 2007; An 2008; Joyce 2008) presented data on cost effectiveness. When full benefit was compared with partial or no benefit, the costs per quitter ranged from USD 97 to USD 7646.

We conclude that full financial interventions directed at smokers when compared to no financial interventions can increase the rate of successful quitting, quit attempts and utilisation of pharmacotherapy in smokers. Although the absolute differences were small, the costs per additional quitter were low. We did not detect a difference in effect between full and partial financial interventions in abstinence from smoking or increased quit attempts, but we did find a significant difference in effect between full and partial interventions in use of NRT and behavioural therapy. Partial versus no financial coverage showed an increase in abstinence and quit attempts and in the use of bupropion and varenicline. There is inadequate evidence to determine the effect of partial financial interventions when compared to other partial financial interventions or no financial interventions on the other outcomes. The conclusions need to be interpreted in light of the reservations discussed below with regard to comparability, classification and methodological quality.

Overall completeness and applicability of evidence

Despite the creation of relatively homogeneous subcategories for analyses based on theoretical and practical considerations, the studies are still heterogeneous with respect to the study setting, motivation to participate in the study and motivation to quit smoking. Because of such sources of heterogeneity, the results of the meta-analysis have to be interpreted with care. The setting of the included studies ranged from family practices and hospital clinics in the UK, USA and Canada, to health insurance organisations in the USA, Germany and the Netherlands. As each country has a different healthcare system, comparisons between studies in various settings should be made in the knowledge of these differences. In Dey 1999, for example, participants had to be motivated to quit in order to participate. On entry into the study, motivated participants received free prescriptions for nicotine patches, and as a result, 97% of the participants in the full incentive group used at least one prescription. Such differences potentially limit the interpretability of the effect size estimates that come out of pooling. On the other hand, Kaper 2006 and Schauffler 2001 offered coverage in a general population. As participants were not enrolled based on motivation to quit smoking, the use of NRT among the intervention groups was 20.6% in Schauffler 2001 and 4% in Kaper 2006. Furthermore, the interventions varied in the extent of financial benefit, the methods of smoking cessation treatment for which the benefit was available, the conditions for receiving the benefit and the information concerning the new benefit. In 12 of the 14 patient-directed financial intervention studies, a financial benefit was available for different types of smoking cessation treatment. In two studies (Halpin 2006 and Twardella 2007), the trial arms did not enable us to examine the independent effect of financial interventions and hence were excluded from the analysis. In Twardella 2007, one of the arms involved both patient-directed and healthcare-directed interventions, while in Halpin 2006, special constraints were put on participants where they were required to attend counselling before free medications were financed.

During subgroup analyses, the main emphasis was on the level of access to smoking cessation treatments: full, partial or no access through financing to enable smokers to quit. Partial interventions could refer to either pharmacotherapy or counselling only. In our subgroup analyses no further stratification was considered for partial interventions for smoking cessation since, in all studies, partial interventions included pharmacotherapy only except Joyce 2008. As a result there is limited confounding by type of treatment provided in the partial intervention group. However, future systematic reviews, with the hope that additional studies would accumulate, need to further stratify partial treatment benefits highlighting the type of smoking cessation treatments covered, at least based on behavioural interventions and pharmacotherapy. This is due to the fact that there could be differences in effect between counselling and pharmacotherapy. It is fair to expect that the kind of pharmacotherapy covered could also influence the outcomes of the financing interventions. All patient-directed studies included in this review covered NRT while nine studies (Boyle 2002; Halpin 2006; Kaper 2006; Twardella 2007; Joyce 2008; Papadakis 2011; Willemsen 2013; Selby 2014; Pakhale 2015) also covered bupropion therapy and four studies additionally covered varenicline (Papadakis 2011; Willemsen 2013; Selby 2014; Pakhale 2015).

An example of different conditions for receiving a financial benefit is related to voluntary or obligatory visits to healthcare providers. In Kaper 2006, participants received coverage after a statement of contact with a healthcare provider was sent to the health insurance company. The use of behavioural support in Kaper 2006 is therefore not comparable with Schauffler 2001 or Joyce 2008, in which some or all of the participants voluntarily chose to participate in a behavioural support intervention. There were also differences in the information provided to participants about their benefit and the extension of the benefit. In Boyle 2002, for example, participants were not explicitly informed of the intervention, and as a result only 30% of smokers in the treatment group knew about the offer of financial benefit. This may be the reason for the statistically non-significant effects found in Boyle 2002 when

compared with other studies, in which participants were informed about their new benefit. Participants' awareness of the available benefits could contribute significantly to an increase in the effect of the intervention, as it would likely increase the use of smoking cessation treatments and hence increase the absolute number of quitters.

The term 'full intervention' should be interpreted with caution as it is not synonymous with optimal level of care. Furthermore, an intervention classified as partial does not exclude the possibility that some motivated participants may have used additional treatment to complement those financed (e.g. Schauffler 2001). This phenomenon has also been documented by other investigators (Hall 2002). As utilisation of smoking cessation treatment was loosely investigated using patient and physician reports or care provider registries, the actual utilisation rates of the services may be lower or higher. It is also difficult to estimate adherence to a prescribed pharmacotherapy from the included studies. As a result, such complexities could have influenced the efficacy of the interventions.

Quality of the evidence

An important limitation of the included studies was that not all used random or concealed allocation. Twelve studies randomly allocated the participants to the treatment or control groups, and only five of these employed concealed allocation. Particularly in Boyle 2002 and Curry 1998, there is the possibility of selection bias as the studies were natural experiments. In the remaining studies, the possibility exists that the effect is biased. Furthermore, not all of the studies blinded the participants, healthcare providers or outcome assessors. In studies that assess the effect of a financial intervention, blinding the control group may be important, since control participants who knew that they would not receive a financial benefit for treatment might have felt disadvantaged and changed their behaviour. Such a change would be a threat to the validity of the study. We therefore could not rule out the possibility of biased results in the unblinded studies. However, we also acknowledge that financial interventions may be more difficult to blind than drug interventions.

Another methodological issue in the included studies is the low follow-up rate, which was below 80% in most of the included studies. This may be related to the type of intervention. If, for example, participants are less interested in the financial intervention when they do not want to use smoking cessation treatments or are not motivated to quit, then the number of dropouts could increase. In a conservative analysis, dropouts would be considered to be continuing smokers. However, such analyses were not performed in five studies (Dey 1999; Schauffler 2001; Boyle 2002; Roski 2003; Twardella 2007). For this review, we have rectified the problem by recalculating the results of the studies using an ITT analysis that counted dropouts and participants lost to follow-up as continuing smokers who had not attempted to quit and not used the treatment(s) offered.

Eight studies reported abstinence outcomes based on self-reported smoking status, although guidelines recommend the use of biochemically validated outcomes (West 2005). Only six studies reported biochemically validated smoking cessation (Dey 1999; Kaper 2006; Twardella 2007; Patel 2010; Papadakis 2011; Selby 2014), but Dey 1999 was not eligible for inclusion into the meta-analysis for the abstinence comparison because follow-up was terminated at 14 weeks. Fortunately, the two studies that assessed the effect of full financing interventions (Kaper 2006; Twardella 2007) (for which the effect of financial interventions was significant) reported longer-term biochemically validated abstinence rates, and hence, have lower risk of bias in this respect. It is possible that reliance on self-report could have introduced bias in the rest of the studies, as participants who were benefiting from free treatment might be more likely to give socially desirable answers than those in the control group.

From the methodological quality assessment of the economic evaluations, it became clear that only one of the studies reporting on cost effectiveness had performed a full economic evaluation (Kaper 2006). Results were presented in terms of costs per additional quitter or costs per person enrolled. Only one study examined cost effectiveness in terms of quality-adjusted life years (QALY) saved (Kaper 2006). As a result, no comparisons can be made with economic evaluations of other preventive healthcare treatments.

Agreements and disagreements with other studies or reviews

This is the only review to assess the effects of financial interventions directed at healthcare providers and smokers to encourage the prescription and use of smoking cessation treatments. We found four reviews (Bains 1998; Hamilton 2013; Cahill 2014; Cahill 2015) examining the effects of financial interventions, but they included studies that offered a financial benefit for abstinence rather than coverage of the cost of smoking cessation treatment. Bains 1998 and Cahill 2015 discussed the use and impact of incentives in population-based smoking cessation programmes. Smokers participated in contests and lotteries or received financial incentives. Cahill 2014 addressed the effectiveness of workplace interventions for smoking cessation. Hamilton 2013 examined the effect of providing financial incentives to healthcare professionals on the provision and impact of smoking cessation interventions. They did not find sufficient evidence to show an effect of financial incentives for healthcare professionals on a reduction in smoking rates.

AUTHORS' CONCLUSIONS

Implications for practice

In this review, covering the full cost to smokers of using smoking cessation treatment increased the number of participants making a quit attempt, the use of smoking cessation treatment, and the number of successful quitters, when compared with no financial coverage. As the majority of the studies were rated at high or unclear risk of bias in three or more domains, and there was variation between the settings, interventions and participants of the included studies, the results should be interpreted cautiously. The differences in self-reported abstinence rate, number of participants making a quit attempt and use of smoking cessation treatments were modest. However, considering the large population of smokers worldwide and the severe health risks of smoking, even modest effects of financial coverage of treatment on smoking cessation could have a substantial effect on public health. The results of this review suggest that full interventions, which cover the cost of pharmacotherapy in combination with behavioural therapy are most promising. This review did not detect an effect of financial interventions directed

at healthcare providers on smoking cessation, which suggests that financial interventions may be more beneficial when allocated to smokers directly. However, due to the small number of studies on financial interventions directed at healthcare providers included in this review it may be premature to draw conclusions based on these results.

Implications for research

More randomised controlled trials should be performed that are comparable with the studies that are included in this review so that future analyses can be stratified by setting, intervention and participants. This is also true for interventions directed at healthcare providers as only three randomised trials are available thus far. More emphasis needs to be put on appropriate reporting of the primary endpoint of smoking cessation, particularly biochemical validation and long-term quitting rates (six months or longer). More randomised trials should assess whether financial interventions aimed at healthcare providers can affect the prescribing pattern and uptake of smoking cessation treatments in addition to the smoking behaviour of their participants. Furthermore, full economic evaluations need to be performed to aid academics, policy makers and stakeholders alike. To assess the financial impact of healthcare financing interventions for smoking cessation, it is important to determine the cost effectiveness of these interventions more precisely. A full economic evaluation is needed to enable comparison of cost effectiveness with other preventive healthcare treatments. We also recommend use of a standard definition and classification of healthcare financing interventions, particularly those directed at healthcare providers, as this could facilitate future intervention research, between-studies comparison, and rational allocation of resources.

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CHARACTERISTICS OF STUDIES

Characteristics of included studies *[ordered by study ID]*

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* Indicates the major publication for the study

An 2008

Methods	Setting: physician network of 49 clinics in the US state of Minnesota from 2005-2006 Design: RCT
Participants	49 clinics providing adult primary care services
Interventions	The 24 clinics which were randomised to the intervention group received a USD 5000 pay-for-performance bonus if they referred 50 smokers to a national telephone quitline, and USD 25 for each additional referral. At the end of the contract period, incentive payments were made in one lump sum directly to the clinics, not to the physicians or other staff. 25 clinics were randomised to a no-intervention control group. In both groups smokers had to express an intention to quit in the next 30 days
Outcomes	The percentage of clinic's smokers referred to telephone counselling defined as the number of unique individuals referred divided by the estimated number of smokers seen in the clinic
Notes	In a post-hoc analysis, the primary outcome was calculated stratified by the level of clinic's history of engagement with quality improvement activities: very engaged, engaged, and less engaged.

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Low risk	Clinics were randomly assigned to an intervention and control group.
Allocation concealment (selection bias)	Unclear risk	Differences at baseline between the characteristics of the usual care and intervention clinics were statistically not significant. However, the percentage of clinics that had a history of being very engaged in quality improvement (i.e. the factor that modified the association between intervention and outcome) was higher in the intervention group (21%) than in the control group (16%). This absolute difference might have been statistically significantly different with a larger sample size.
Blinding (performance bias and detection bias) All outcomes	High risk	Clinic directors and administrators were not blinded as to group assignment of their clinic.
Incomplete outcome data (attrition bias) All outcomes	Low risk	No attrition occurred as the measurement of the primary outcome was not participant dependent.
Other bias	Unclear risk	Only a process-based outcome was reported (i.e. percentage of smokers referred to a quit line) rather than preferable targets such as smoking cessation.

Boyle 2002

Methods	Setting: employer groups insured at the Blue Cross Blue Shield of Minnesota and Health Partners, USA, in 1999 Design: comparative study, natural experiment
Participants	Treatment group: 767. Control group: 1560 Smokers identified by postal questionnaire who were willing to participate in two postal surveys Exclusions: < 100 cigarettes a lifetime, unclear health insurance status, already quit smoking or unable to complete the survey because of illness Av. age 46, F = 55.9%; daily smokers 91%, 46% interested in quitting over next 30 days

Boyle 2002 *(Continued)*

Interventions	Treatment group: introduction of coverage (only with a provider's prescription) for nicotine gum, nicotine patch and bupropion as part of insurance benefits. Counseling was not covered. Control group: no coverage (self-insured employer groups who chose not to offer pharmacy benefit to employees)
Outcomes	a) Self-reported continuous abstinence (no smoking for last 6 months+) b) Self-reported quit attempt for at least 1 day c) Utilisation of smoking cessation treatment
Notes	In the treatment group only 30.3% were aware of the pharmacy benefit. No economic evaluation was performed. Volunteer bias a potential threat. The study authors did not provide data for ITT analysis. Intervention categorised as partial coverage in review update 2009 because no behavioural counselling was available.

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	High risk	Smokers were not randomly assigned to receive the benefit. Study compared smokers in insured employer groups receiving different benefits.
Allocation concealment (selection bias)	High risk	Not directly applicable; benefit groups were not experimentally manipulated. Response rate to baseline survey identical across conditions, measured characteristics not significantly different except more people smoked within 5 min of waking in benefit group
Blinding (performance bias and detection bias) All outcomes	High risk	Not applicable, participants did not know the nature of the study
Incomplete outcome data (attrition bias) All outcomes	High risk	Participants were self -elected respondents to a survey so an ITT analysis was not possible. In addition the authors did not account for 20% "unusable sample" excluded from analysis
Other bias	High risk	Self-reports of quitting were not biochemically validated.

Curry 1998

Methods	Setting: consumer-owned HMO (Group Health Cooperative of Puget Sound), Washington, USA, 1993-4 Design: comparative study, natural experiment with four coverage groups
Participants	Service users (denominators for cessation rates): standard coverage (controls) n = 158. Full coverage n = 130. Flipped coverage n = 27. Reduced coverage n = 113. Smokers covered by plans (estimates from surveys): 6133; 2767; 1769; 6253. Enrollees in Group Health Cooperative aged 18-64 yrs; av age 42; F = 53%
Interventions	1. Standard coverage group: 50% co-payment for the behavioural programme and full coverage of NRT 2. Full coverage group: full coverage of the behavioural programme and full coverage of NRT 3. Flipped coverage: full coverage of the behavioural programme and a 50% co-payment for NRT 4. Reduced coverage group: a 50% co-payment for the behavioural programme and a 50% co-payment for NRT A payment of USD 5 per prescription was not included in the coverage
Outcomes	a) Self-reported 7-day PP abstinence at 6 months, for the behavioural participants only

Curry 1998 *(Continued)*

	b) Automated data collection of the use of smoking cessation treatment, for behavioural participants only
Notes	This comparative study estimated the use of smoking services and smoking cessation using different samples. All the participants for cessation estimation were selected among service users. Hence service usage among all study groups was 100% for counselling, while NRT use differed from group to group. Comparison of abstinence between full coverage and reduced coverage (50% coverage for both NRT and the behavioural programme) after a year Comparison of use of smoking cessation treatment, between full and flipped coverage groups, and between full and standard coverage groups after a year An economic evaluation was performed using the third party payer perspective and users' perspective. The costs per benefit user who quit smoking were reported.

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	High risk	Study compared groups of employer-based health maintenance programmes that offered different levels of cover for smoking cessation treatment. Randomisation not employed
Allocation concealment (selection bias)	High risk	Not directly applicable; benefit groups were not experimentally manipulated. Participants were self-selected service users.
Blinding (performance bias and detection bias) All outcomes	High risk	Participants knew their benefit group.
Incomplete outcome data (attrition bias) All outcomes	Low risk	There was unbalanced missing data. Quote: "...and the overall response rate was 81%. The numbers of respondents and response rates for the four groups were as follows: standard coverage, 130 (82%); reduced coverage, 94 (83%); flipped coverage, 23 (85%); and full coverage, 98 (75%)." The study authors used an ITT analysis including non respondents.
Other bias	Unclear risk	Inadequate description of the different samples employed

Dey 1999

Methods	Setting: general practices in East Lancashire, UK, in 1996 Design: RCT
Participants	Treatment group n = 64. Control group n = 58 Age range 25-64 years; av. age 43; F = 56%. Participants were motivated to quit smoking; cpd > 15
Interventions	Treatment group: free prescriptions for 12 weeks of nicotine patches Control group: 12 weeks of nicotine patches at slightly reduced retail price
Outcomes	a) Biochemically validated abstinence from 8-14 weeks; salivary cotinine level < 14 ng/mL, CO level < 10 ppm at 14 weeks b) Use of smoking cessation treatment (cashing in one or more NRT prescriptions) No economic evaluation was performed.
Notes	Study not used for assessing impact on abstinence because follow-up period was less than 6 months

Risk of bias

Dey 1999 *(Continued)*

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Low risk	Quote: "..were then randomly allocated by an off-site randomisation office.." Comment: probably done
Allocation concealment (selection bias)	Low risk	Quote: "All subjects received standardised brief counselling and were then randomly allocated by an off-site randomisation office.." Comment: probably done since enrolment preceded allocation
Blinding (performance bias and detection bias) All outcomes	High risk	No information was provided on blinding of participants. Comment: probably not done
Incomplete outcome data (attrition bias) All outcomes	High risk	Quote: "The differential response rate is disappointing." Missing interventions group 6/64; control group 19/58. Reasons for drop out not provided
Other bias	High risk	The study reported only short-term abstinence rate (3 months) in addition to being a pilot study in nature (and hence low power as compared to the rest of the included studies)

Halpin 2006

Methods	Setting: California HMOs, USA, in 2001 Design: RCT
Participants	Control group: n = 126. Treatment group 1: n = 140. Treatment group 2: n = 127 Age range: 18-50+, F = 66%. The drugs were a 'USD 15' low cost co-payment. Counselling does not involve cost sharing.
Interventions	Treatment group 1: bupropion and NRT patch, inhaler and nasal spray and proactive telephone counselling Treatment group 2: if enrolled for counselling then pharmacotherapy coverage Control: pharmacotherapy only: coverage for bupropion, and NRT patch, inhaler and nasal spray
Outcomes	a) Self-reported PP abstinence at 8 months b) Cost per additional quitter
Notes	Treatment group 2 was excluded from analysis, as it added a restriction

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Low risk	The study mentioned randomisation though short of further explanation. Comment: probably done
Allocation concealment (selection bias)	Unclear risk	No information provided
Blinding (performance bias and detection bias) All outcomes	High risk	Inadequate description of blinding with regard to healthcare providers, participants or outcome assessors. Comment: probably not done
Incomplete outcome data (attrition bias)	Low risk	ITT was conducted though there was limited characterisation of the more or less balanced (18%) missing data.

Halpin 2006 *(Continued)*

All outcomes		
Other bias	Unclear risk	No biochemical validation of quitting

Hughes 1991

Methods	Setting: 2 rural family practices, Vermont, USA, probably 1989/1990 Design: RCT.
Participants	Treatment group 1: n = 32. Treatment group 2: n = 36. Control group: n = 38 Participants aged 18+, av age 37.7 years; F = 41.9%; av 26.2 cpd; no previous use of nicotine gum
Interventions	Treatment group 1: full coverage for nicotine gum Treatment group 2: partial coverage, and nicotine gum USD 6 per box Control group: (almost) no coverage, and nicotine gum at USD 20 per box All participants also received brief quit smoking advice according to the 5 As (Ask, Advise, Assess, Assist, and Arrange)
Outcomes	a) Self-reported 6 months PP abstinence (77% "observer" validated) b) Self-reported quit attempts at 6 months c) Utilisation of smoking cessation treatment, by prescription dates and number of unused gum pieces An economic evaluation was performed according to a third party payer perspective. The costs were presented per subject enrolled. The monetary benefits from smoking cessation were also calculated.
Notes	

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	Low risk	Quote: "Smokers were randomly assigned.." Comment: probably done
Allocation concealment (selection bias)	Low risk	Participants were enrolled and physicians gave advice before opening sealed envelopes. Although envelopes not specified to be numbered or opaque judged to be low risk of bias
Blinding (performance bias and detection bias) All outcomes	High risk	Quote: "since the physicians knew the price each subject was paying for gum, the physicians could have biased the study by encouraging cessation more in the free-gum group."
Incomplete outcome data (attrition bias) All outcomes	Low risk	There were 19.8% missing participants. An ITT was conducted.
Other bias	Unclear risk	PP was ascertained with "observer verification".

Jardin 2014

Methods	Setting: South Carolina, USA, probably 2012/2013 Design: RCT, when comparing group UNQ (unmotivated, NRT, quitline) with group UQ (unmotivated, quitline)

Jardin 2014 *(Continued)*

Participants	Current cigarette smokers of ≥ 10 cpd Intervention group: n = 53, control group: n = 51, av age: intervention group: 44.6; control group: 43.9
Interventions	Treatment group: 2-week supply of 14 mg nicotine patch and 4 mg lozenge plus referral to the South Carolina state quitline Control group: quitline referral only
Outcomes	a) 24-h quit attempts b) Use of NRT c) Use of behavioral therapy
Notes	New for 2017 update. Funding: quote: "This study was supported by the Hollings Cancer Center and National Institute on Drug Abuse grants R01 CA141663 (PI: Cropsey) and K23 DA020482 (PI: Carpenter)". Conflicts of interest: quote: "KMG has received funding from Merck Inc. and Supernus Pharmaceuticals for unrelated research. KMC serves as a paid expert witness in litigation against the tobacco industry. He also has received funding support from Pfizer Corporation to build a hospital-based cessation service."

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Low risk	The unmotivated smokers were randomised in an intervention and control group.
Allocation concealment (selection bias)	Unclear risk	Method of concealment not described
Blinding (performance bias and detection bias) All outcomes	Unclear risk	Method of blinding not described
Incomplete outcome data (attrition bias) All outcomes	Low risk	Across 628 (157 × 4) scheduled telephone calls, 96% were completed, and 92% of participants completed all 4 telephone calls. An ITT analysis was performed.

Joyce 2008

Methods	Cluster RCT conducted in seven states in USA
Participants	7354 elderly smokers (aged ≥ 65 years) enrolled in the Medicare Stop Smoking Program
Interventions	(1) usual care (participants received smoking cessation information only), (2) reimbursement for provider counselling, (3) reimbursement for provider counselling with pharmacotherapy (nicotine patch or bupropion), and (4) telephone counselling quitline with pharmacotherapy (nicotine patch)
Outcomes	Primary outcomes: self-reported 7 day PP of non smoking at 6- and 12-months' follow-up Secondary outcomes: self-reported attempts to quit for at least 24 h in the first 6 and 12 months
Notes	The trial was restricted to elderly smokers who had been smoking for over 40 years. Trial participants were not randomised at individual level, but randomisation occurred at geographic locale level of pri-

Joyce 2008 *(Continued)*

	mary care providers. Data were, however, analysed at individual level and not at cluster level (with multi-level analysis) as one would expect.

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Low risk	Randomisation was conducted at geographic locale level of primary care providers.
Allocation concealment (selection bias)	High risk	Benefit groups were manipulated. However there was a risk of bias because participants were not randomised at individual level. The study reports that "enrollees differed on race, education, income, quit attempts, and stage of change. While statistically significant, these differences were absolutely small."
Blinding (performance bias and detection bias) All outcomes	High risk	Participants and providers were not blinded.
Incomplete outcome data (attrition bias) All outcomes	High risk	Attrition rates were 32.5% at 6 months and 39.4% at 12 months. For the primary analysis, these data were imputed (non-respondents counted as smokers). It is unclear whether attrition was at random or not and whether differential attrition occurred (attrition rates were not presented for each of the four intervention groups separately).
Other bias	Unclear risk	Only 7-day self-reported smoking cessation used and these were not biochemically validated

Kaper 2006

Methods	Setting: smokers covered by insurance company 'De Friesland', the Netherlands, 2002 Design: RCT
Participants	Treatment group: n = 632. Control group: n = 634. Participants 18+ years, av age 40 yrs, 55% men. Participants were not required to be motivated to quit.
Interventions	Intervention group: offer of reimbursement for counselling and pharmacotherapy such as bupropion for up to 6 months post-entry
Outcomes	a) Prolonged abstinence, at 12 months biochemically validated b) PP abstinence, self-reported and biochemically validated at 12 months c) Self-reported use of smoking cessation treatment d) Economic evaluation performed
Notes	Previously Kaper 2003. Kaper 2006 provided 12-month follow-up data plus economic evaluation, compared to 6-month report in Kaper 2003

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Low risk	Quote: "were randomised to the intervention or control groups using a computer generated randomisation list."

Kaper 2006 *(Continued)*

Allocation concealment (selection bias)	High risk	Quote: "since the Dutch Institute for Public Opinion and Market Research ('TNS NIPO') [third party] contacted the smokers, the authors did not know the participants when performing randomisation." This is not adequate.
Blinding (performance bias and detection bias) All outcomes	Low risk	Quote: "As we expected that participants in the control group might change their behaviour because they were disappointed, we used a double randomised consent design in order to blind them for the intervention group." In addition, the authors have reported limited knowledge of treatment assignment among a sub-sample of participants surveyed. The situation may not bias the reported endpoints. Comment: participants were blinded.
Incomplete outcome data (attrition bias) All outcomes	Low risk	37% of intervention and 31% of control groups lost to follow-up/dropped out at 6 months. Only quitters followed at 12 months; 2/35 (5.7%) intervention and 3/18 (16.7%) control did not respond. An ITT analysis included all dropouts.
Other bias	High risk	There were limitations in outcome ascertainment. Quote: "Only 33% of the self-reported quitters in the control group could be validated biochemically at the end of the reimbursement period and 6-month follow-up and in the intervention group this was 69%. Because of this difference between the groups, the results of the biochemical validation should be interpreted with care."

Pakhale 2015

Methods	Setting: outpatient respirology clinic of the Ottawa Hospital in Canada, November 2011-December 2012 Design: open-label feasibility RCT study
Participants	49 respirology patients identified as smokers, who were willing to set a quit date within 1 month Av age 59.4, 49% men
Interventions	Treatment group: standard care, brief counselling session, USD 110 voucher for the purchase of NRT, bupropion or varenicline, registration to automated calling system, nurse telephone counselling for participants who relapsed but wanted to make another quit attempt or those with low confidence Control group: standard smoking cessation treatment including strong physician advice, information brochure and prescription of pharmacotherapy on request
Outcomes	a) Self-report smoking status 26-52 weeks b) Use of pharmacotherapy (general) c) Use of NRT
Notes	New for 2017 update. Conflicts of interest: quote: "The authors have no financial disclosures or conflicts of interest to declare."

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Low risk	Quote: "sealed and opaque envelopes were prepared by UOHI using a computer-generated allocation sequence based on stratified (according to sex) block randomisation".

Pakhale 2015 *(Continued)*

Allocation concealment (selection bias)	Low risk	Quote: "sealed and opaque envelopes were prepared".
Blinding (performance bias and detection bias) All outcomes	High risk	Quote: "blinding of participants was not possible due to the nature of the intervention"
Incomplete outcome data (attrition bias) All outcomes	Low risk	Data were collected for 16 of 26 (62%) in the control group and 16 of 23 (70%) in the intervention group. Loss to follow-up ws not statistically different between the control and intervention group. All participants, with the exception of those who were deceased or had moved to an untraceable address, were included in the analysis. Participants with missing self-reported smoking status were considered to be smokers.
Other bias	High risk	Researchers were unable to collect CO samples and could therefore not verify smoking status. The decision was made to forego biochemical confirmation which resulted in delays, and therefore outcome data were collected on different time points for participants, i.e. between 26 and 52 weeks. On average these data were collected 33 weeks after baseline, with no significant difference between study groups.

Papadakis 2011

Methods	Setting: Ottawa Hospital Stroke Prevention Clinic in Canada, probably 2010/2011 Design: cluster-RCT
Participants	28 patients who smoked ≥ five cpd, were ready to quit smoking in the next 30 days, and willing to use pharmacotherapy. Av age 54.5, 60.7% men
Interventions	Treatment group: participants received a starter kit (4-week supply) of cost-free quit-smoking medication (NRT, bupropion or varenicline) and a pre-printed renewal prescription Control group: prescription-only usual care
Outcomes	a) 26-week 7-day PP abstinence, CO validated b) Continuous abstinence measured at 26 weeks (+- 2 weeks) after the target quit date c) 24 h quit attempts
Notes	New for 2017 update. Funding: quote: "No external funding was received for the completion of this pilot study." Competing interests: quote: "The institutions and study authors at no time received payment or services from a third party for any aspect of the work submitted. The University of Ottawa Heart Institute has received research and education grant support from Pfizer Canada, Johnson and Johnson, and GlaxoSmithKline. AP has served as a consultant and has received speaker honoraria from Pfizer, Johnson and Johnson, and GlaxoSmithKline; RR has received speaker honoraria from Pfizer; DA has received speaker honoraria from Pfizer; and DR has served as a consultant to Pfizer."

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Low risk	Quote: "Patients were randomly assigned to one of two intervention groups. Randomisation envelopes were prepared by a third party using a random

Papadakis 2011 *(Continued)*

		numbers table blocked in groups of four and sealed until treatment allocation."
Allocation concealment (selection bias)	Low risk	Quote: "The research coordinator or clinic nurse specialist opened a sealed envelope which contained the treatment group allocation."
Blinding (performance bias and detection bias) All outcomes	High risk	Quote: "Due to the nature of the intervention, participants and clinicians were not blinded to their intervention assignment." The research co-ordinator conducting outcome assessment was blind to group allocation.
Incomplete outcome data (attrition bias) All outcomes	Low risk	5 participants were lost to follow-up (2 in intervention group and 3 in control group). All participants were included in the ITT analysis.
Other bias	Unclear risk	Intervention group participants reported smoking significantly more cpd as well as significantly greater self-efficacy (confidence) with quitting compared to participants randomised to the control group

Patel 2010

Methods	Setting: participants were recruited from Tucson, Arizona, USAand the surrounding area. The study was conducted in an over-the-counter setting. Design: RCT
Participants	270 self-reported smokers, aged > 18 years. (1) Free treatment group: n = 86; (2) USD 10/box, n = 89; (3) USD 20/box, n = 93
Interventions	Groups were defined according to the price at which nicotine polacrilex gum could be acquired from a study clinic. Treatment group 1: free nicotine polacrilex gum Treatment group 2: nicotine polacrilex gum for USD 10/box Treatment group 3: nicotine polacrilex gum for USD 20/box
Outcomes	CO-validated PP abstinence at 26 weeks
Notes	No full research report available, poster was acquired after correspondence with study author New for 2017 update. No information on funding sources and/or competing interests

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Low risk	Quote: "After obtaining the informed consent, each participant was randomly assigned to 1 of 3 groups"
Allocation concealment (selection bias)	Unclear risk	No information on allocation concealment provided
Blinding (performance bias and detection bias) All outcomes	Unclear risk	No information on blinding provided

Patel 2010 *(Continued)*

Incomplete outcome data (attrition bias) All outcomes	High risk	Quote: "Fifty-five percent of subjects assigned to the $20/box arm withdrew from the study prior to the 2-week visit compared with 34.83% of subjects assigned to the $10/box arm and 12 79% of subjects assigned to the $0/box arm. Attrition rates differed significantly among the 3 groups (p < 0.001)."
Other bias	Unclear risk	Study was not published as a full report, data only available via research poster

Roski 2003

Methods	Setting: 40 clinics of a multispecialty medical group practice, in Washington DC, USA in 1999-2000 Design: Cluster-RCT with 3 groups of which 2 are included in the review
Participants	Treatment group: n = 13 clinics. Control group: n = 15 clinics A postal survey was used to identify 3436 smokers and recent ex-smokers aged ≥ 18 years with clinic visits after the start of intervention. 2729 were surveyed by telephone after 6 months.
Interventions	Treatment group: guideline dissemination to clinics, financial incentives for reaching preset clinical performance targets Control group: guideline dissemination
Outcomes	a) Self-reported 6 months PP abstinence (i.e. no smoking previous 7 days) b) Utilisation of smoking cessation treatment
Notes	No corrections were made for clustering

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	Low risk	Quote: "A three-condition group randomised efficient (unbalanced) evaluation design was employed." Comment: probably done
Allocation concealment (selection bias)	Unclear risk	Participants were identified from a postal survey of clinic attenders. No information about baseline characteristics to judge whether participants similar across conditions
Blinding (performance bias and detection bias) All outcomes	High risk	No information is provided on the blinding of assessments surveys; the study authors; or the healthcare practice centres. In fact the study authors were communicating the performance of the practices in general using leaflets and charts, this may put limitation on any blinding that may have been implemented.
Incomplete outcome data (attrition bias) All outcomes	High risk	19.5% not reached at 6 months or excluded from analyses. No clear information on how non-response or losses to follow-up were handled in the analysis. The study authors only report "response rates did not differ by experimental condition."
Other bias	High risk	Quote: "substantial higher incentive payments for changes in targeted clinical practices might have threatened the generalisability of this study. The introduction of practice monitoring systems by performing chart audits and the introduction of the exit interviews may have had a Hawthorne effect on all clinics regardless of their randomisation status." In addition, baseline comparability was not reported.

Schauffler 2001

Methods	Setting: 16 large companies offering employee health benefits from 2 California HMOs, California, USA, in 1998 Design: RCT, pre-test post-test assessment surveys
Participants	Treatment group: n = 601. Control group: n = 603 Participants aged ≥ 18 years, current smokers, smoked > 100 cigarettes in their lifetime Demographic data not reported, but no significant differences detected between 2 study arms
Interventions	Treatment group: free self-help kit, 4 free orders of nicotine gum or patches during 1 year and coverage of a behaviour group programme Control group: free self-help kit only
Outcomes	a) Self-reported 7 day PP at 12 months b) Self-reported quit attempt (i.e. not having smoked for ≥ 1 d over the 12 months) c) Utilisation of smoking cessation treatment An economic evaluation was performed according to a third party payer perspective
Notes	

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Low risk	Quote: "...subjects were randomly assigned to the control or treatment group." Comment: probably done
Allocation concealment (selection bias)	Unclear risk	No information was provided on the concealment of the randomisation schedule
Blinding (performance bias and detection bias) All outcomes	High risk	No information was provided on blinding. Comment: probably not done
Incomplete outcome data (attrition bias) All outcomes	High risk	Missing data (26% control and 27% of the intervention group) were not well addressed. An ITT was not reported, but all participants were included in the denominators in the meta-analysis.
Other bias	Unclear risk	Study authors reported a high possibility of a selective group of participants.

Selby 2014

Methods	Setting: 58 different ambulatory care settings across Canada, March 2009-September 2010 Design: open-label RCT
Participants	Participants were adults (18–75 years) who smoked ≥ 10 cpd, were willing to set a quit date within 14 d following screening/randomisation, had no period of abstinence > 3 months in the past year, and had not attempted to quit smoking in the 30-day period before screening. Intervention group: n = 696, control group: n = 684, Av age intervention group: 46.5, control group: 46.7. Gender intervention group: 50.9% men, control group: 49.3% men
Interventions	Treatment group: full coverage of prescribed pharmacotherapy for 26 weeks. Eligible pharmacotherapies were: varenicline; bupropion; and NRT patch and gum.

Selby 2014 *(Continued)*

	Control group: no coverage
Outcomes	a) Urine cotinine-confirmed 7-day PP abstinence b) Urine cotinine-confirmed continuous abstinence at weeks 26-52 c) Having made at least 1 quit attempt
Notes	New for 2017 update. Funded by Pfizer Inc. Competing interests: quote: "Pfizer sponsored the study and recruited study sites based on various factors, including but not limited to, clinical research experience and capabilities, smoking cessation expertise/interest, and referral from peers. Data were analyzed by Pfizer and made available to the authors for interpretation and preparation of the manuscript. PS, GB, and PO did not receive honoraria for their participation or for writing the manuscript. VR, CA, and SR are employees of Pfizer Inc. PO, PS, and GB declare financial compensation from Pfizer Inc. for professional services, including protocol and clinical trial materials development, initial start-up, and end-of-study activities such as Case Report Form review, Statistical Analysis Plan review, preparation, participation, and presentation at the Investigator Meeting, and Clinical Study Report review. No payments were made by Pfizer Inc. to PO, PS, or GB for authorship and/or authorship-related activities of this paper. CA, SR, and VR are employees of and shareholders in Pfizer Inc."

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Low risk	Quote: "Randomization was performed by the investigators using blinded lots of computer-generated randomisation codes from the study biostatistician."
Allocation concealment (selection bias)	Low risk	Quote: "Randomization codes were mapped to SmartPayment™ cards (drug reimbursement cards), with a distinctive colour linked to the study arm to which the subject was randomised. The SmartPayment™ cards were enclosed in sealed envelopes with the randomisation codes printed on the envelopes."
Blinding (performance bias and detection bias) All outcomes	High risk	Investigators/subjects were unblinded to study group assignment after randomisation and prior to choosing a smoking cessation method(s). Regarding blinding of outcome assessment, the risk of bias was considered low: quote: "Confirmatory urinary cotinine measurements were collected, but investigators and patients were blinded to the results to mimic real-world practice"
Incomplete outcome data (attrition bias) All outcomes	High risk	222 (31.9%) of the intervention group and 267 (39.0%) of the control group discontinued the study. Reasons were reported and the reason 'no longer willing to participate' was reported more often in the control group. The primary analysis was ITT and involved all participants who were randomly assigned.

Twardella 2007

Methods	Setting: 82 medical practices in Germany, including 94 general practitioners, in 2006 Design: cluster-randomised trial, 2 x 2 factorial design
Participants	Usual care: n = 76. TI: n = 146. TM: n = 144. TI+TM: n = 221 587 people who smoked at least 10 cpd and aged 36-75 years, F = 5.2%
Interventions	TI: provision of a 2-h physician group training in SC methods and direct physician payments for every participant not smoking 12 months after recruitment TM: provision of the same training and direct participant reimbursement for pharmacy costs associated with NRT or bupropion treatment

Twardella 2007 *(Continued)*

Outcomes	a) Self-reported and biochemically validated PP abstinence at 12 months b) Biomedically validated CA at 6 months
Notes	GPs were not randomised. Some mistakes in calculations. TI+TM group was not used in analyses, since effect of intervention could not be separated

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Low risk	Quote: "Randomisation was performed centrally..." Comment: probably done
Allocation concealment (selection bias)	High risk	Potential for selection bias: participants recruited after practices randomised, number of participants per practice variable. Some practices dropped out or failed to recruit any participants. There were baseline differences; "regarding the stage of change for smoking cessation: in arms TM and TI+TM, the proportion of participants in the pre-contemplation stage - that is, participants with no concrete intention to stop smoking - was lower, and the proportion of participants in both the contemplation and preparation stages was higher than in the usual care and TI arms"
Blinding (performance bias and detection bias) All outcomes	High risk	Quote: "Owing to the nature of the interventions, general practitioners and participants could not be blinded to the intervention."
Incomplete outcome data (attrition bias) All outcomes	High risk	Even though missing data were considered smokers there is inadequate description and no statistical analysis on differences in missing data between groups. The authors mention ITT strategy, but did not implement it in the analysis.

Willemsen 2013

Methods	Setting: Dutch national smoking cessation quitline 2010-2012 Design: descriptive time-series analysis
Participants	Smokers signing up for proactive counselling in 2011
Interventions	Starting from January 2011 and ending in December 2011, Dutch smokers were eligible for full reimbursement of a smoking cessation programme, consisting of behavioural treatment, preferably combined with pharmacotherapy.
Outcomes	The number of smokers who started a telephone counselling programme during the intervention period in 2011 compared to pre- and post-intervention in 2010 and 2012
Notes	New for 2017 update. Funding sources: not stated. Competing interests: none

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Unclear risk	Not applicable

Willemsen 2013 *(Continued)*

Allocation concealment (selection bias)	Unclear risk	Not applicable
Blinding (performance bias and detection bias) All outcomes	Unclear risk	Not applicable
Incomplete outcome data (attrition bias) All outcomes	Unclear risk	Not applicable
Other bias	High risk	There was also a media campaign when the reimbursement policy was implemented, so it is not possible to disentangle the effects of the reimbursement policy and the media campaign.

Av: average (mean); CA: continuous abstinence; cpd: cigarettes per day; CO: carbon monoxide; F: female; **HMO**: Healthcare Maintenance OrganisationNRT: nicotine replacement therapy; PP: point prevalence; SC: Smoking cessation; ITT: Intention to treat; TI: Training +incentive; TM: Training+ medication; .

Characteristics of excluded studies *[ordered by study ID]*

Study	Reason for exclusion
Alberg 2004	Not a RCT or ITS
Amemori 2013	Outcome measure was delivery of counselling (did not meet inclusion criteria)
Amundson 2003	Not a RCT or ITS
Bailey 2016	Not a RCT or ITS
Bardach 2013	Outcome measure was delivery of counselling (did not meet inclusion criteria)
Bonevski 2016	Study protocol. Also, the effect of direct coverage of treatment was not examined separately
Chang 2008	Not a RCT or ITS
Coleman 2001	Not a RCT or ITS
Courtney 2014	Study protocol. Also, both intervention and control group received free smoking cessation treatment
Cox 1990	Not a RCT or ITS
Cummings 2006	Used a quite disparate historical control. Not RCT or ITS
Curry 1991	The financial incentive was not related to the use of smoking cessation treatment
Doescher 2002	Not a RCT or ITS
Donatelle 2000	The financial incentive was not related to the use of smoking cessation treatment
Fiore 2000	Not a RCT or ITS
Fu 2016	The trial studied the effect of proactive outreach tobacco treatment

Study	Reason for exclusion
Hamilton 2013	Not a RCT or ITS
Harter 2015	Study protocol. Furthermore, the effect of a financial incentive was not assessed
Hays 1999	The effect of a financial incentive was not assessed
Hockenberry 2012	The effect of a financial incentive was not assessed
Hovell 1996	Trial was to prevent adolescent smoking, not for cessation. Russos 1999 is a secondary publication of data from this trial
Krist 2010	Not a RCT or ITS. The free counselling intervention was not directed at smoking specifically but also at other 'unhealthy' behaviours such alcohol drinking and overweight
Kruse 2013	Not a RCT or ITS
Land 2010	Design did not meet inclusion criteria: There were 15 time points before the intervention and 5 time points after the intervention. But for quit attempts and quit success the number of time points was unclear. In the results section it wa stated that “information about quit success was not asked in every year”. In table 1, quit attempts and quit success are compared between before the intervention (2003-June 2006) and after (Jan-Dec 2008); which suggests that there were only one or two time points after the intervention for these outcomes.
Latts 2002	Not a RCT or ITS
Lave 1996	No data were available for the control group
McLeod 2015	Not a RCT or ITS
Millett 2007	Not RCT or ITS design
Moskowitz 2016	The financial incentive was not related to smoking cessation treatment
Oswald 1988	Not a RCT or ITS
Pardell 2003	The financial incentive was not related to the use of smoking cessation treatment
Park 2016	Study protocol. Furthermore, the free medication arm was confounded with additional counselling
Parnes 2002	Not a RCT or ITS
Ringen 2002	Not a RCT or ITS
Russos 1999	The financial incentive was not related to the use of smoking cessation treatment. This is a secondary publication of Hovell 1996
Shaw 2003	Data concerning the outcome measures are not yet available
Silverman 2004	Study was never published, study authors were not able to provide data
Solberg 2002	Not a RCT or ITS
Stone 2002	Not a RCT or ITS
Verbiest 2013	Outcome measure was number of prescribed medications, not participant-level use of medications

Study	Reason for exclusion
Volpp 2006	Quit and win strategy was mixed with financial interventions (did not meet inclusion criteria)
Walsh 2012	Outcome measures did not meet inclusion criteria
Weisman 2012	Not a RCT or ITS

ITS: interrupted time series; **RCT**: randomised controlled trial

Characteristics of ongoing studies *[ordered by study ID]*

NCT00962988

Trial name or title	Efficacy and cost-effectiveness of cost-free pharmacotherapy for smoking cessation for high-risk smokers with cerebrovascular disease
Methods	Setting: the Ottawa Hospital stroke prevention clinic, Canada Design: RCT
Participants	Smokers with transient ischemic attack (TIA) or stroke attending a stroke prevention clinic and willing to quit smoking
Interventions	Treatment group: cost-free pharmacotherapy Control group: prescription-only usual care group
Outcomes	a) Biochemically confirmed (exhaled CO < 10 ppm) self-reported continuous abstinence from weeks 12-52 following the target quit date. b) Biochemically confirmed (exhaled CO < 10 ppm) self-reported continuous abstinence from weeks 12-26 following the target quit date c) Cost-effectiveness of providing cost-free pharmacotherapy for smoking cessation versus a prescription only
Starting date	December 2009
Contact information	Dr Robert Reid, Ottawa Heart Institute Research Corporation
Notes	

Ostroff 2014

Trial name or title	Implementing tobacco use treatment guidelines in dental public health clinics Dentistry United to Extinguish Tobacco (DUET)
Methods	Setting: dental clinics in the NYC metropolitan area, USA Design: cluster-RCT
Participants	a) Clinics are included if they are located within the NYC metropolitan area and employ at least 3 FTE dentists. b) Providers are included if they practice full-time or part-time at one of the study clinics.

Ostroff 2014 *(Continued)*	
	c) Participants are included if they are ≥ 18 years, active smokers defined as those who report smoking cigarettes some days, most days, or every day and have smoked in the past 7 days, have an appointment with a dentist or hygienist, NYS resident, speak English, Spanish, Chinese or Russian, and are able to comply with study procedures in the opinion of the principal investigator.
Interventions	Treatment group: will receive current best practices (CBP), quarterly audit and performance feedback reports (PF), financial incentives (pay for performance, P4P) for every documented delivery of adherence to clinical practice guidelines. Control group: CBP + PF
Outcomes	a) Patient utilisation of cessation services b) Smoking abstinence c) Cost analysis
Starting date	February 2013
Contact information	ostroffj@mskcc.org
Notes	

RCT: randomised controlled trial

DATA AND ANALYSES

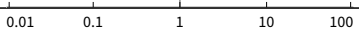
Comparison 1. Interventions directed at individuals: abstinence from smoking

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Full versus no financial coverage	6	9333	Risk Ratio (M-H, Random, 95% CI)	1.77 [1.37, 2.28]
1.1 Continuous abstinence (at least 6 months)	2	1486	Risk Ratio (M-H, Random, 95% CI)	4.38 [1.94, 9.87]
1.2 Point prevalence abstinence	4	7847	Risk Ratio (M-H, Random, 95% CI)	1.64 [1.45, 1.86]
2 Full versus partial financial coverage	5	5914	Risk Ratio (M-H, Random, 95% CI)	1.02 [0.71, 1.48]
2.1 Continuous abstinence (at least 6 months)	1	28	Risk Ratio (M-H, Random, 95% CI)	2.17 [0.50, 9.35]
2.2 Point prevalence abstinence	4	5886	Risk Ratio (M-H, Random, 95% CI)	0.98 [0.67, 1.44]
3 Partial versus no financial coverage	5	7108	Risk Ratio (M-H, Random, 95% CI)	1.27 [1.02, 1.59]
3.1 Continuous abstinence (at least 6 months)	2	3707	Risk Ratio (M-H, Random, 95% CI)	1.08 [0.81, 1.45]
3.2 Point prevalence	3	3401	Risk Ratio (M-H, Random, 95% CI)	1.49 [0.98, 2.24]

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
4 Partial versus another partial coverage (at least 6 months CA)	1		Risk Ratio (M-H, Fixed, 95% CI)	Totals not selected

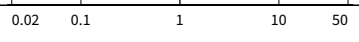
Analysis 1.1. Comparison 1 Interventions directed at individuals: abstinence from smoking, Outcome 1 Full versus no financial coverage.

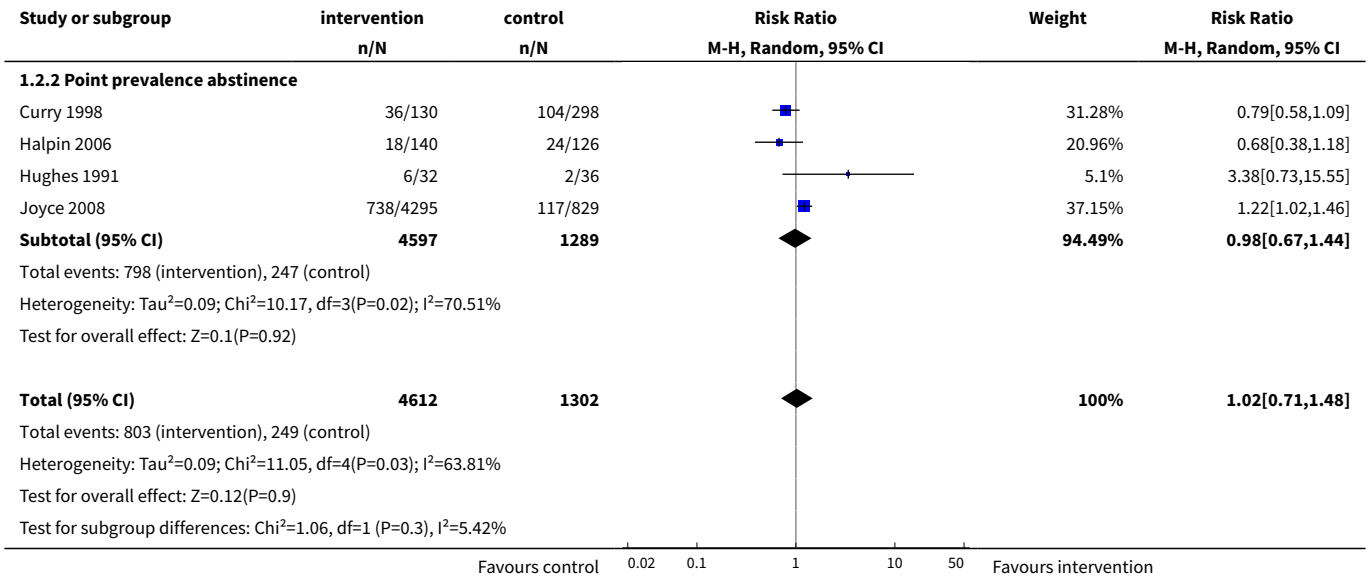
Study or subgroup	intervention n/N	control n/N	Risk Ratio M-H, Random, 95% CI	Weight	Risk Ratio M-H, Random, 95% CI
1.1.1 Continuous abstinence (at least 6 months)					
Kaper 2006	24/632	6/634		7.34%	4.01[1.65,9.75]
Twardella 2007	13/144	1/76		1.58%	6.86[0.91,51.46]
Subtotal (95% CI)	**776**	**710**		**8.92%**	**4.38[1.94,9.87]**
Total events: 37 (intervention), 7 (control)					
Heterogeneity: Tau^2=0; Chi^2=0.23, df=1(P=0.63); I^2=0%					
Test for overall effect: Z=3.56(P=0)					
1.1.2 Point prevalence abstinence					
Hughes 1991	6/32	3/38		3.64%	2.38[0.64,8.75]
Joyce 2008	738/4295	227/2230		51.53%	1.69[1.47,1.94]
Pakhale 2015	4/22	2/26		2.47%	2.36[0.48,11.7]
Schauffler 2001	91/601	65/603		33.45%	1.4[1.04,1.89]
Subtotal (95% CI)	**4950**	**2897**		**91.08%**	**1.64[1.45,1.86]**
Total events: 839 (intervention), 297 (control)					
Heterogeneity: Tau^2=0; Chi^2=1.72, df=3(P=0.63); I^2=0%					
Test for overall effect: Z=7.75(P<0.0001)					
Total (95% CI)	**5726**	**3607**		**100%**	**1.77[1.37,2.28]**
Total events: 876 (intervention), 304 (control)					
Heterogeneity: Tau^2=0.03; Chi^2=7.45, df=5(P=0.19); I^2=32.9%					
Test for overall effect: Z=4.34(P<0.0001)					
Test for subgroup differences: Chi^2=5.47, df=1 (P=0.02), I^2=81.7%					



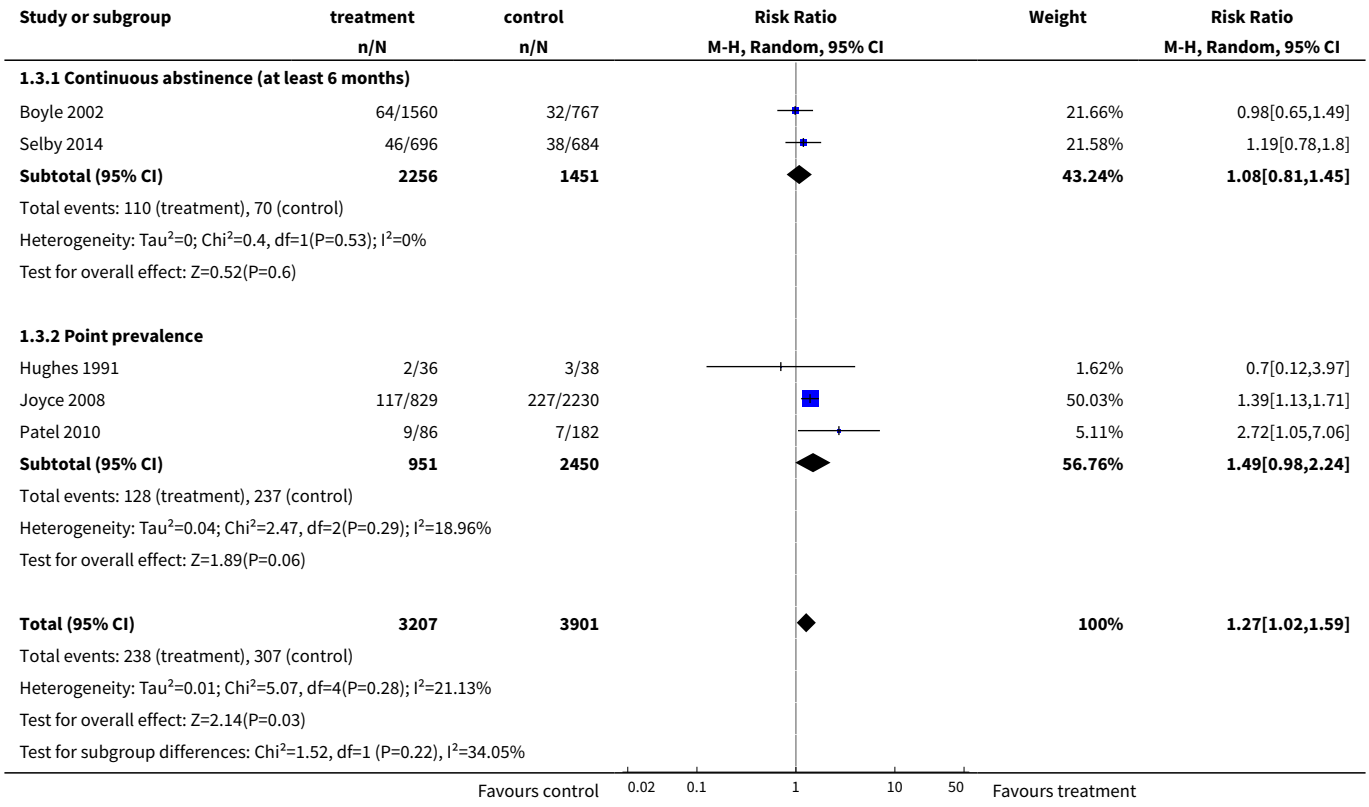
Analysis 1.2. Comparison 1 Interventions directed at individuals: abstinence from smoking, Outcome 2 Full versus partial financial coverage.

Study or subgroup	intervention n/N	control n/N	Risk Ratio M-H, Random, 95% CI	Weight	Risk Ratio M-H, Random, 95% CI
1.2.1 Continuous abstinence (at least 6 months)					
Papadakis 2011	5/15	2/13		5.51%	2.17[0.5,9.35]
Subtotal (95% CI)	**15**	**13**		**5.51%**	**2.17[0.5,9.35]**
Total events: 5 (intervention), 2 (control)					
Heterogeneity: Not applicable					
Test for overall effect: Z=1.04(P=0.3)					



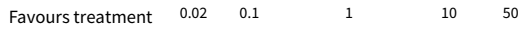


Analysis 1.3. Comparison 1 Interventions directed at individuals: abstinence from smoking, Outcome 3 Partial versus no financial coverage.



Analysis 1.4. Comparison 1 Interventions directed at individuals: abstinence from smoking, Outcome 4 Partial versus another partial coverage (at least 6 months CA).

Study or subgroup	Treatment n/N	Control n/N	Risk Ratio M-H, Fixed, 95% CI	Risk Ratio M-H, Fixed, 95% CI
Curry 1998	69/185	35/113		1.2[0.86,1.68]

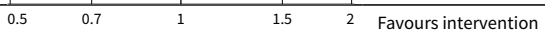


Comparison 2. Interventions directed at individuals: number of participants making a quit attempt for at least 24 h

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Full versus no financial coverage	4	9065	Risk Ratio (M-H, Random, 95% CI)	1.11 [1.04, 1.17]
2 Full versus partial financial coverage	4	5486	Risk Ratio (M-H, Random, 95% CI)	0.99 [0.84, 1.17]
3 Partial versus no financial coverage	5	6944	Risk Ratio (M-H, Random, 95% CI)	1.13 [0.98, 1.31]

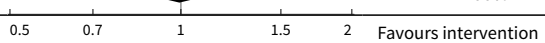
Analysis 2.1. Comparison 2 Interventions directed at individuals: number of participants making a quit attempt for at least 24 h, Outcome 1 Full versus no financial coverage.

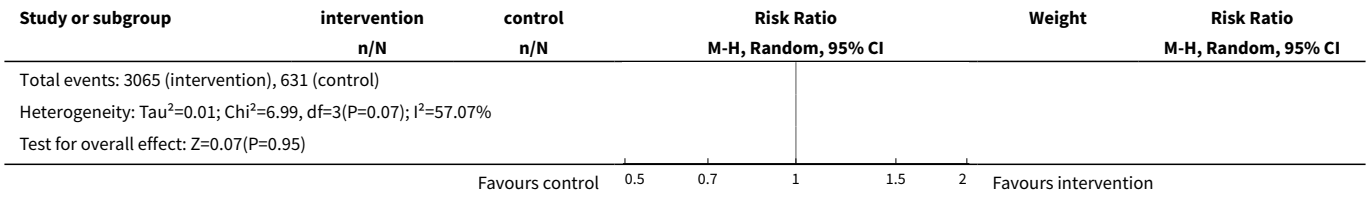
Study or subgroup	intervention n/N	control n/N	Risk Ratio M-H, Random, 95% CI	Weight	Risk Ratio M-H, Random, 95% CI
Hughes 1991	27/32	25/38		4.12%	1.28[0.98,1.69]
Joyce 2008	2970/4295	1431/2230		73.44%	1.08[1.04,1.12]
Kaper 2006	148/632	132/634		6.99%	1.12[0.91,1.38]
Schauffler 2001	275/601	232/603		15.45%	1.19[1.04,1.36]
Total (95% CI)	**5560**	**3505**		**100%**	**1.11[1.04,1.17]**
Total events: 3420 (intervention), 1820 (control)					
Heterogeneity: Tau^2=0; Chi^2=3.51, df=3(P=0.32); I^2=14.62%					
Test for overall effect: Z=3.46(P=0)					



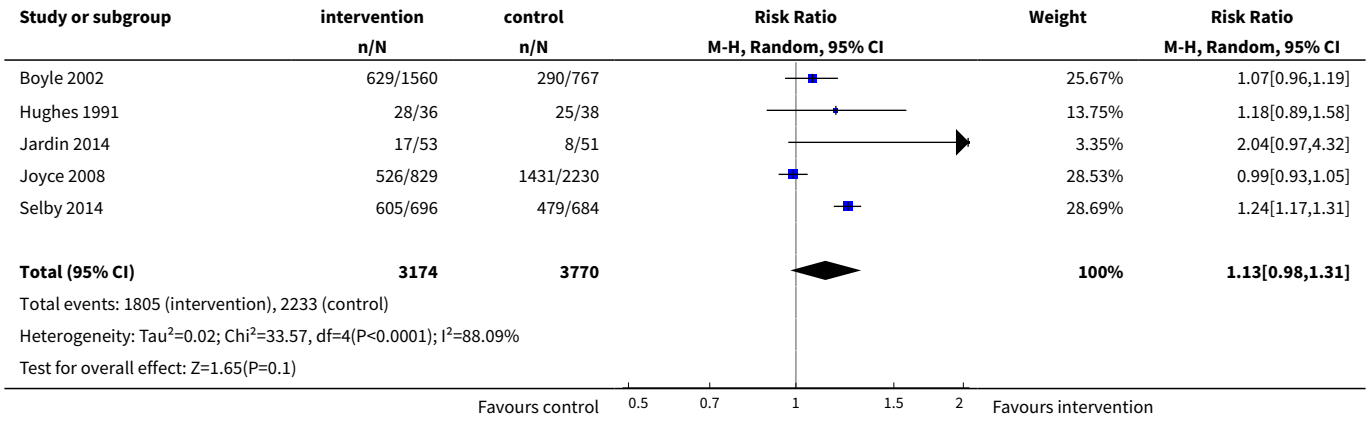
Analysis 2.2. Comparison 2 Interventions directed at individuals: number of participants making a quit attempt for at least 24 h, Outcome 2 Full versus partial financial coverage.

Study or subgroup	intervention n/N	control n/N	Risk Ratio M-H, Random, 95% CI	Weight	Risk Ratio M-H, Random, 95% CI
Halpin 2006	60/140	69/126		23.2%	0.78[0.61,1]
Hughes 1991	27/32	28/36		25.11%	1.08[0.86,1.36]
Joyce 2008	2970/4295	526/829		45.76%	1.09[1.03,1.15]
Papadakis 2011	8/15	8/13		5.93%	0.87[0.46,1.64]
Total (95% CI)	**4482**	**1004**		**100%**	**0.99[0.84,1.17]**





Analysis 2.3. Comparison 2 Interventions directed at individuals: number of participants making a quit attempt for at least 24 h, Outcome 3 Partial versus no financial coverage.



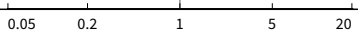
Comparison 3. Interventions directed at individuals: use of smoking cessation treatment

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Full versus no financial coverage	7		Risk Ratio (M-H, Random, 95% CI)	Subtotals only
1.1 Nicotine replacement therapy	7	9455	Risk Ratio (M-H, Random, 95% CI)	1.79 [1.54, 2.09]
1.2 Bupropion	3	6321	Risk Ratio (M-H, Random, 95% CI)	3.22 [1.41, 7.34]
1.3 Behavioural interventions	4	9215	Risk Ratio (M-H, Random, 95% CI)	1.77 [1.19, 2.65]
1.4 Pharmacotherapy, not specified	1	48	Risk Ratio (M-H, Random, 95% CI)	1.11 [0.73, 1.68]
2 Full versus partial financial coverage	5		Risk Ratio (M-H, Random, 95% CI)	Subtotals only
2.1 Nicotine replacement therapy	4	22380	Risk Ratio (M-H, Random, 95% CI)	1.76 [1.27, 2.43]

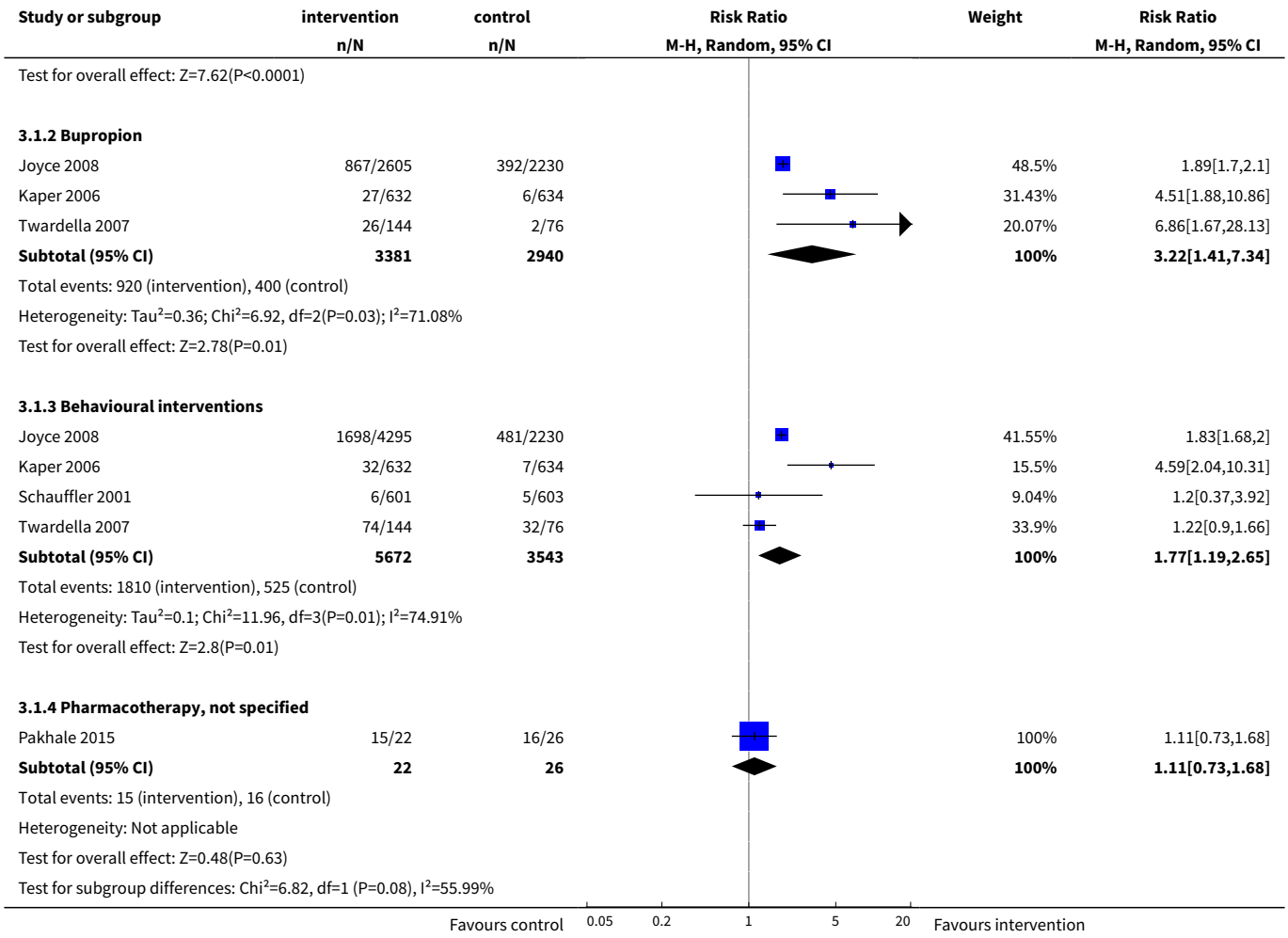
Outcome or subgroup title	No. of studies	No. of partici-pants	Statistical method	Effect size
2.2 Bupropion	2	3700	Risk Ratio (M-H, Random, 95% CI)	1.42 [0.84, 2.41]
2.3 Behavioural interventions	1	16922	Risk Ratio (M-H, Random, 95% CI)	3.95 [3.15, 4.95]
2.4 Pharmacotherapy, not specified	1	28	Risk Ratio (M-H, Random, 95% CI)	1.19 [0.70, 2.02]
3 Partial versus no financial coverage	5		Risk Ratio (M-H, Random, 95% CI)	Subtotals only
3.1 Nicotine replacement therapy	5	6944	Risk Ratio (M-H, Random, 95% CI)	1.37 [0.99, 1.91]
3.2 Bupropion	3	6765	Risk Ratio (M-H, Random, 95% CI)	1.15 [1.03, 1.29]
3.3 Varenicline	1	1380	Risk Ratio (M-H, Random, 95% CI)	1.85 [1.68, 2.03]
3.4 Behavioural interventions	1	104	Risk Ratio (M-H, Random, 95% CI)	0.77 [0.22, 2.71]
4 Partial versus partial financial coverage	1		Risk Ratio (M-H, Random, 95% CI)	Totals not selected
4.1 Nicotine replacement therapy	1		Risk Ratio (M-H, Random, 95% CI)	0.0 [0.0, 0.0]
4.2 Behavioural interventions	1		Risk Ratio (M-H, Random, 95% CI)	0.0 [0.0, 0.0]

Analysis 3.1. Comparison 3 Interventions directed at individuals: use of smoking cessation treatment, Outcome 1 Full versus no financial coverage.

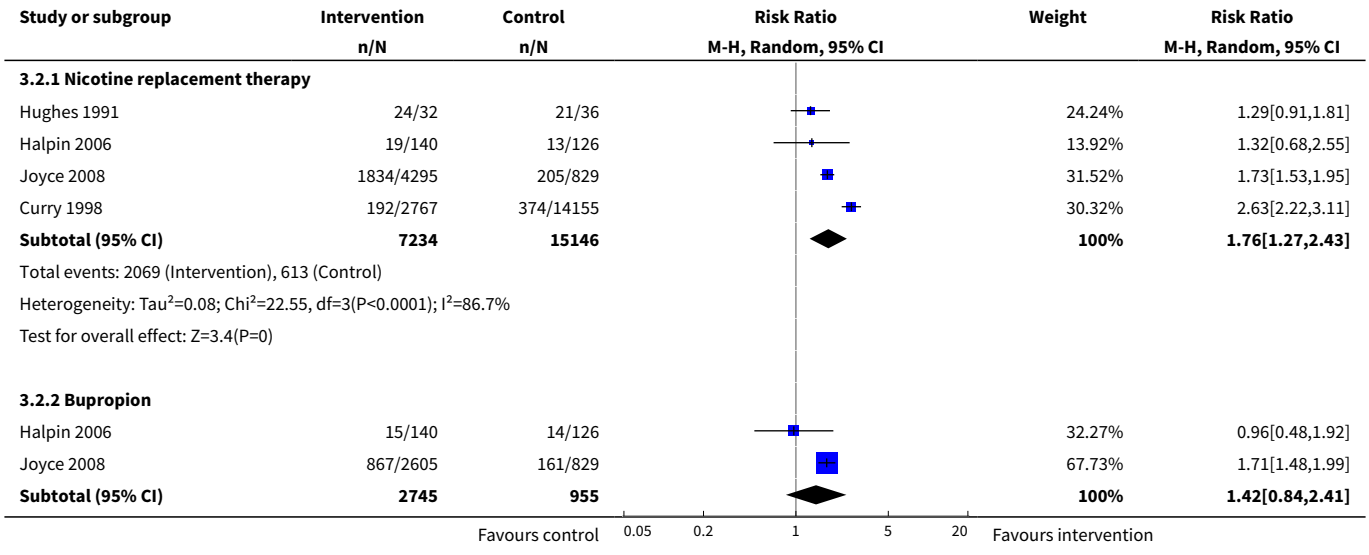
Study or subgroup	intervention n/N	control n/N	Risk Ratio M-H, Random, 95% CI	Weight	Risk Ratio M-H, Random, 95% CI
3.1.1 Nicotine replacement therapy					
Dey 1999	62/64	28/58		18.86%	2.01[1.53,2.63]
Hughes 1991	24/32	18/38		11.35%	1.58[1.07,2.34]
Joyce 2008	1834/4295	575/2230		42.52%	1.66[1.53,1.79]
Kaper 2006	23/632	6/634		2.69%	3.85[1.58,9.38]
Pakhale 2015	9/22	8/26		3.58%	1.33[0.62,2.86]
Schauffler 2001	124/601	70/603		18.83%	1.78[1.36,2.33]
Twardella 2007	33/144	4/76		2.16%	4.35[1.6,11.83]
Subtotal (95% CI)	**5790**	**3665**		**100%**	**1.79[1.54,2.09]**
Total events: 2109 (intervention), 709 (control)					
Heterogeneity: Tau²=0.01; Chi²=9.13, df=6(P=0.17); I²=34.3%					

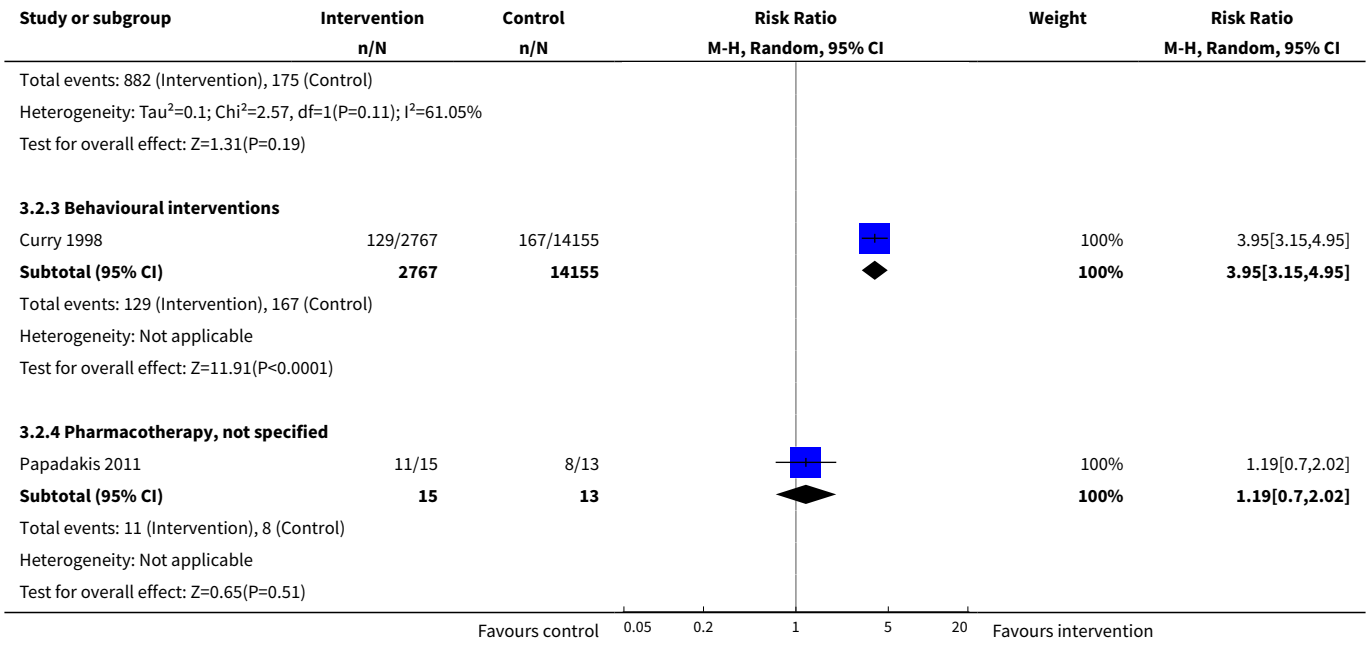


Favours control — Favours intervention

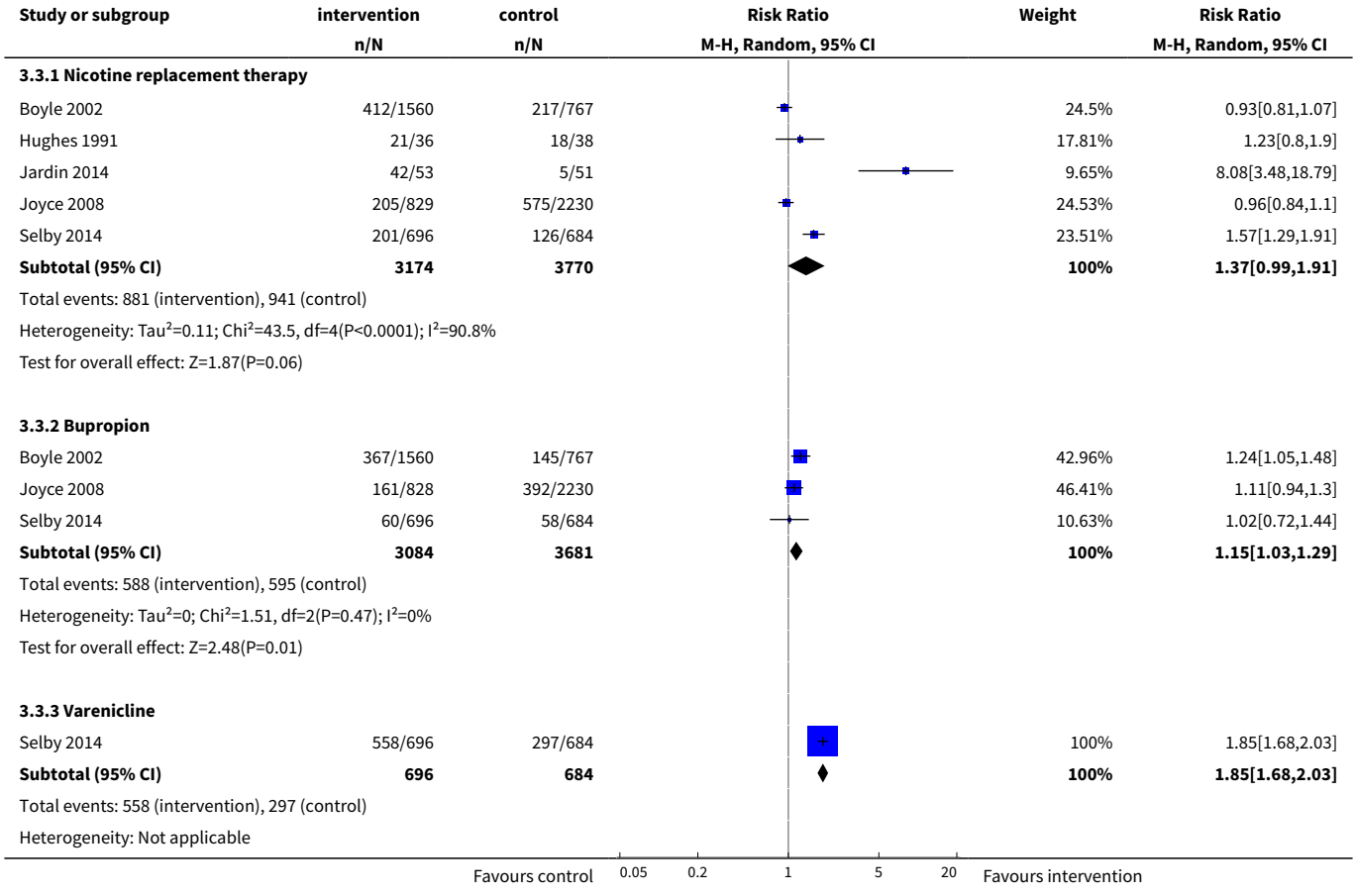


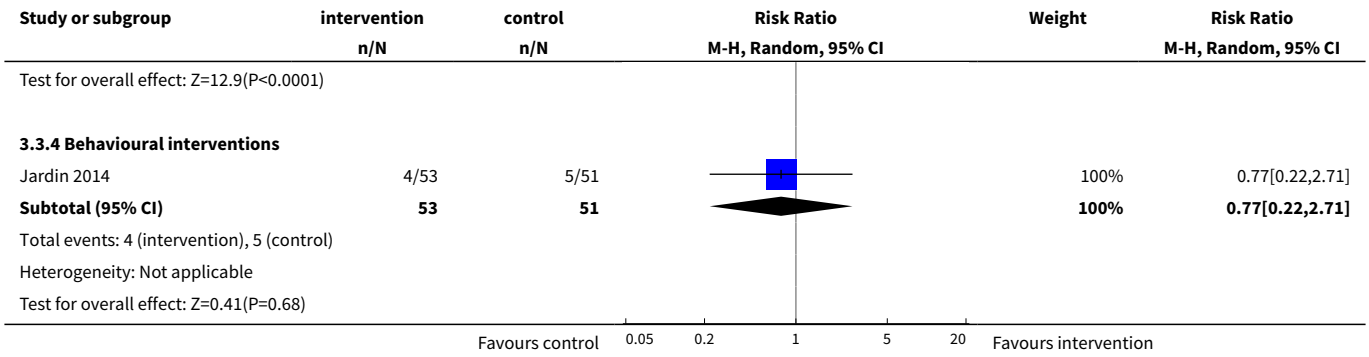
Analysis 3.2. Comparison 3 Interventions directed at individuals: use of smoking cessation treatment, Outcome 2 Full versus partial financial coverage.



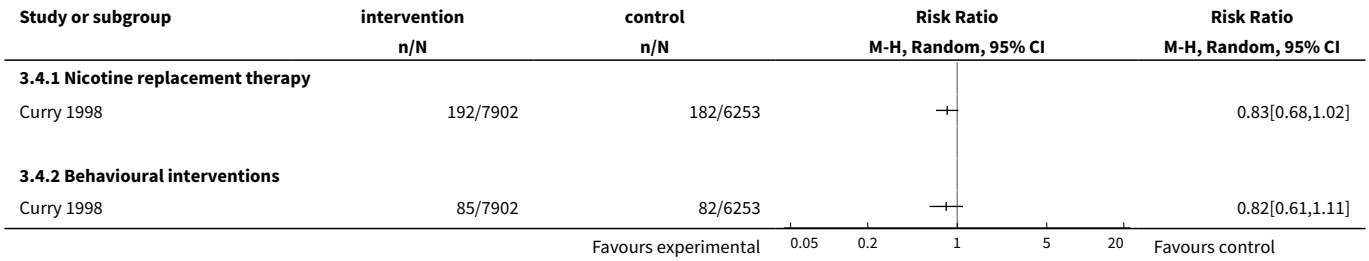


Analysis 3.3. Comparison 3 Interventions directed at individuals: use of smoking cessation treatment, Outcome 3 Partial versus no financial coverage.





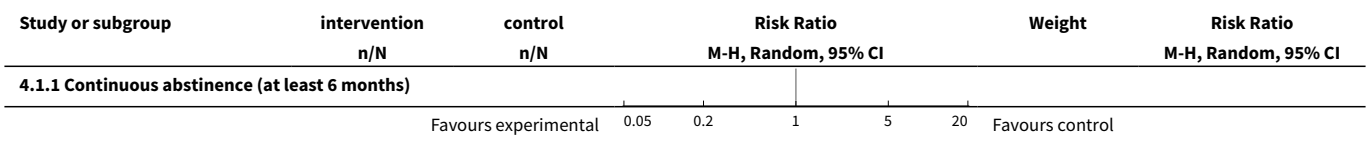
Analysis 3.4. Comparison 3 Interventions directed at individuals: use of smoking cessation treatment, Outcome 4 Partial versus partial financial coverage.

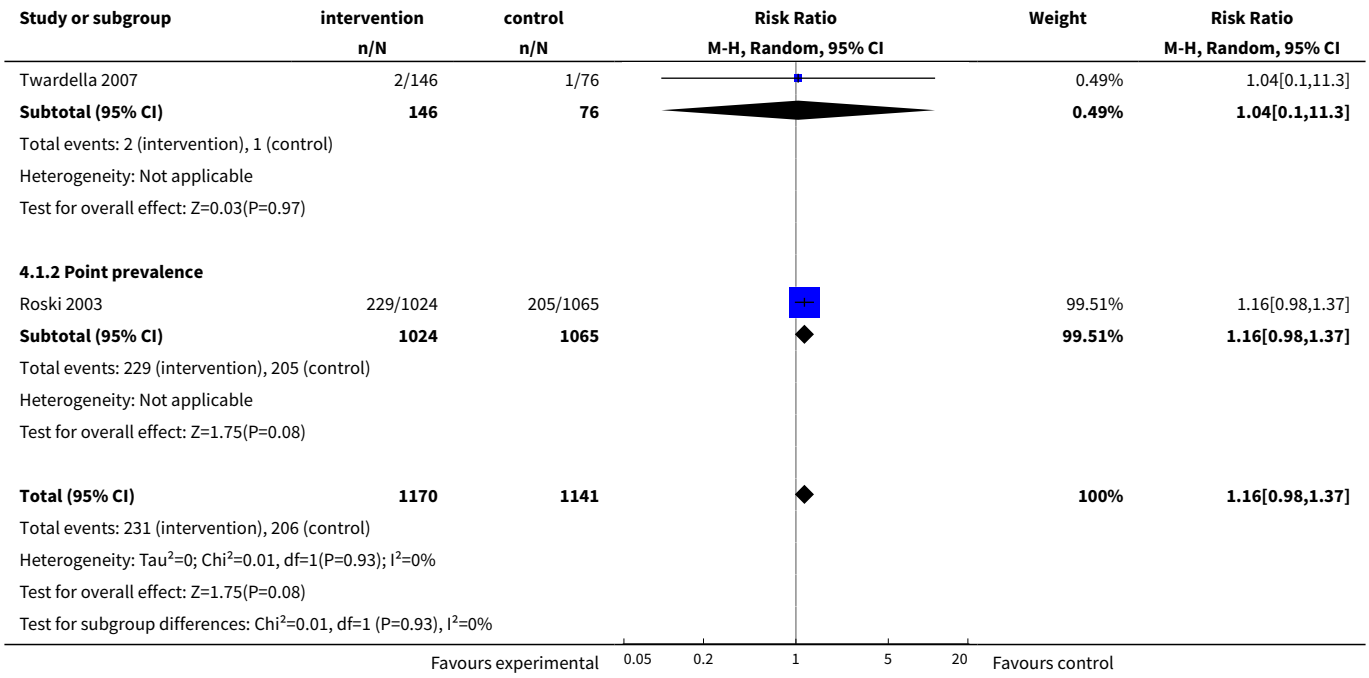


Comparison 4. Interventions directed at healthcare providers

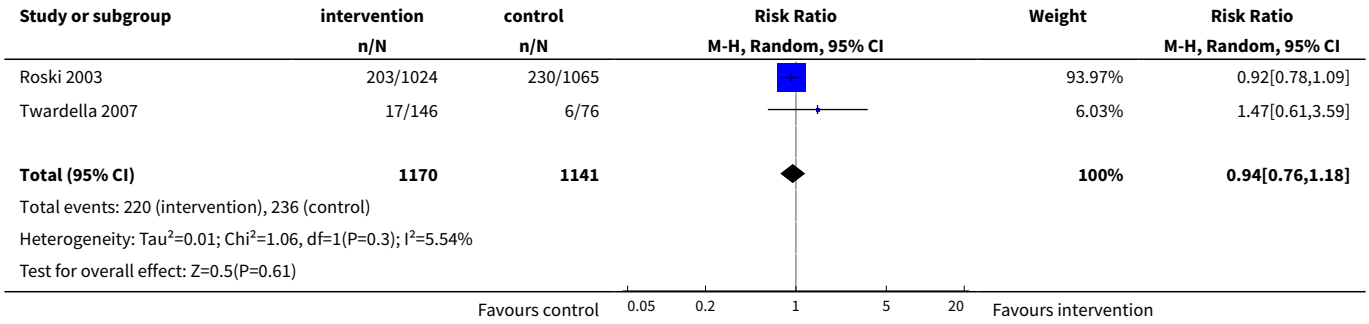
Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Abstinence from smoking	2	2311	Risk Ratio (M-H, Random, 95% CI)	1.16 [0.98, 1.37]
1.1 Continuous abstinence (at least 6 months)	1	222	Risk Ratio (M-H, Random, 95% CI)	1.04 [0.10, 11.30]
1.2 Point prevalence	1	2089	Risk Ratio (M-H, Random, 95% CI)	1.16 [0.98, 1.37]
2 Use of nicotine replacement therapy and/or bupropion	2	2311	Risk Ratio (M-H, Random, 95% CI)	0.94 [0.76, 1.18]
3 Use of behavioural interventions	3	25820	Risk Ratio (M-H, Random, 95% CI)	1.69 [1.01, 2.86]

Analysis 4.1. Comparison 4 Interventions directed at healthcare providers, Outcome 1 Abstinence from smoking.

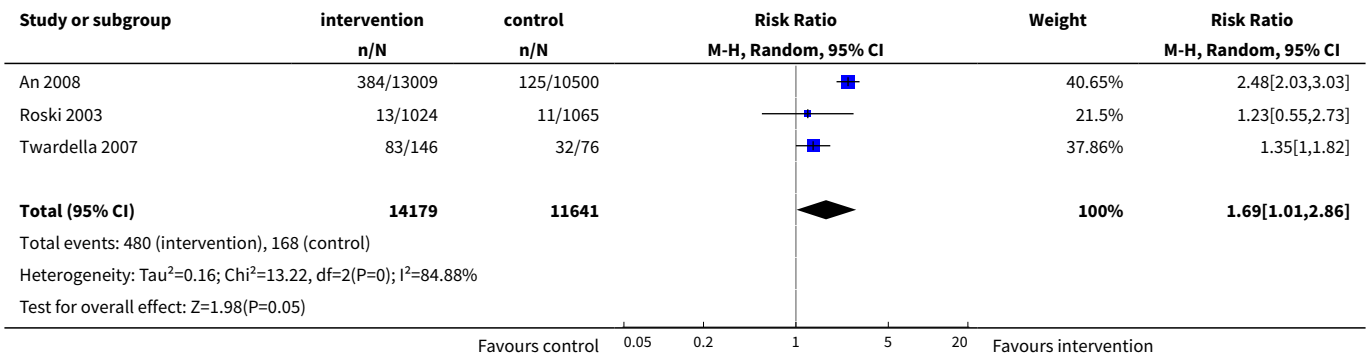




Analysis 4.2. Comparison 4 Interventions directed at healthcare providers, Outcome 2 Use of nicotine replacement therapy and/or bupropion.



Analysis 4.3. Comparison 4 Interventions directed at healthcare providers, Outcome 3 Use of behavioural interventions.



APPENDICES

Appendix 1. Specialised Register Search Strategy

Search strategy used for Specialised Register (using Cochrane Register of Studies (CRS) software)

#1 MeSH DESCRIPTOR Insurance Explode All

#2 MeSH DESCRIPTOR Insurance Coverage Explode All

#3 MeSH DESCRIPTOR Insurance, Health Explode All

#4 MeSH DESCRIPTOR Reimbursement Mechanisms Explode All

#5 MeSH DESCRIPTOR Insurance, Health, Reimbursement Explode All

#6 MeSH DESCRIPTOR social control policies Explode All

#7 MeSH DESCRIPTOR health care costs Explode All

#8 MeSH DESCRIPTOR Quality of Health Care Explode All

#9 MeSH DESCRIPTOR Fee-for-Service Plans Explode All

#10 MeSH DESCRIPTOR Physician Incentive Plans Explode All

#11 MeSH DESCRIPTOR Costs and Cost Analysis Explode All

#12 MeSH DESCRIPTOR Cost-Benefit Analysis Explode All

#13 health care costs

#14 health insurance

#15 coverage*:AB,TI

#16 reimburse*

#17 payment*

#18 remunerat*

#19 incentive*

#20 salary or salaries

#21 fee or fees

#22 deductible*

#23 co?insurance

#24 co?payment

#25 capita*

#26 fund?hold*

#27 prepay or prepaid

#28 financ* NEAR incentive*:AB,TI

#29 cost? NEAR (shar* or free or no):TI,AB

#30 #1 OR #2 OR #3 OR #4 OR #5 OR #6 OR #7 OR #8 OR #9 OR #10 OR #11 OR #12 OR #13 OR #14 OR #15 OR #16 OR #17 OR #18 OR #19 OR #20 OR #21 OR #22 OR #23 OR #24 OR #25 OR #26 OR #27 or #28

Appendix 2. MEDLINE search strategy

The following topic related terms were combined with MeSH and free text terms concerning smoking and tobacco use, and with terms to identify trials and other evaluations of healthcare effects used by Cochrane Tobacco Addiction for regular searches of MEDLINE. (See Specialized Register section of Tobacco Addiction Group Module). The free text term 'time series' was included in the trials identification set.

exp Insurance, Health, Reimbursement/ or exp Insurance/ or exp Insurance Coverage/ or exp Insurance, Health/ or exp Reimbursement Mechanisms/ or exp Insurance, Health, Reimbursement/ or exp social control policies/ or exp health care costs/ or "Quality of Health Care"/ ec or exp Fee-for-Service Plans/ or exp Managed Care Programs/ or exp Physician Incentive Plans/ or exp Employee Incentive Plans/ or (coverage or reimburs$ or target$ or payment$ or remunerat$ or incentive$ or financ$ or salar$ or fee or fees or deductible$ or coinsurance or copayment or capita$ or cost$ or payment$ or fundhold$ or prepay$ or prepaid).mp. [mp=title, original title, abstract, name of substance word, subject heading word]

Appendix 3. Glossary of terms

Term	Definition
Abstinence	A period of being quit, i.e. stopping the use of cigarettes or other tobacco products, may be defined in various ways; see also: point prevalence abstinence; prolonged abstinence; continuous/sustained abstinence
Biochemical verification	Also called 'biochemical validation' or 'biochemical confirmation'. A procedure for checking a tobacco user's report that he or she has not smoked or used tobacco. It can be measured by testing levels of nicotine or cotinine or other chemicals in blood, urine, or saliva, or by measuring levels of carbon monoxide in exhaled breath or in blood.
Bupropion	A pharmaceutical drug originally developed as an antidepressant, but now also licensed for smoking cessation; trade names Zyban, Wellbutrin (when prescribed as an antidepressant)
Carbon monoxide (CO)	A colourless, odourless highly poisonous gas found in tobacco smoke and in the lungs of people who have recently smoked, or (in smaller amounts) in people who have been exposed to tobacco smoke. May be used for biochemical verification of abstinence.
Cessation	Also called 'quitting' The goal of treatment to help people achieve abstinence from smoking or other tobacco use, also used to describe the process of changing the behaviour
Continuous abstinence	Also called 'sustained abstinence' A measure of cessation often used in clinical trials involving avoidance of all tobacco use since the quit day until the time the assessment is made. The definition occasionally allows for lapses. This is the most rigorous measure of abstinence
'Cold turkey'	Quitting abruptly, and/or quitting without behavioural or pharmaceutical support
Craving	A very intense urge or desire [to smoke]. See: Shiffman et al 'Recommendations for the assessment of tobacco craving and withdrawal in smoking cessation trials' Nicotine & Tobacco Research 2004: 6(4): 599-614
Dopamine	A neurotransmitter in the brain that regulates mood, attention, pleasure, reward, motivation and movement
Efficacy	Also called 'treatment effect' or 'effect size' The difference in outcome between the experimental and control groups
Harm reduction	Strategies to reduce harm caused by continued tobacco/nicotine use, such as reducing the number of cigarettes smoked, or switching to different brands or products, e.g. potentially reduced exposure products (PREPs), smokeless tobacco

(Continued)

Lapse/slip	Terms sometimes used for a return to tobacco use after a period of abstinence. A lapse or slip might be defined as a puff or two on a cigarette. This may proceed to relapse, or abstinence may be regained. Some definitions of continuous, sustained or prolonged abstinence require complete abstinence, but some allow for a limited number or duration of slips. People who lapse are very likely to relapse, but some treatments may have their effect by helping people recover from a lapse.
nAChR	Neural nicotinic acetylcholine receptors Areas in the brain that are thought to respond to nicotine, forming the basis of nicotine addiction by stimulating the overflow of dopamine
Nicotine	An alkaloid derived from tobacco, responsible for the psychoactive and addictive effects of smoking.
Nicotine replacement therapy (NRT)	A smoking cessation treatment in which nicotine from tobacco is replaced for a limited period by pharmaceutical nicotine. This reduces the craving and withdrawal experienced during the initial period of abstinence while users are learning to be tobacco-free. The nicotine dose can be taken through the skin, using patches, by inhaling a spray, or by mouth using gum or lozenges.
Outcome	Often used to describe the result being measured in trials that is of relevance to the review. For example smoking cessation is the outcome used in reviews of ways to help smokers quit. The exact outcome in terms of the definition of abstinence and the length of time that has elapsed since the quit attempt was made may vary from trial to trial.
Pharmacotherapy	A treatment using pharmaceutical drugs, e.g. NRT, bupropion
Point prevalence abstinence (PPA)	A measure of cessation based on behaviour at a particular point in time, or during a relatively brief specified period, e.g. 24 hours, 7 days. It may include a mixture of recent and long-term quitters. See prolonged abstinence, continuous abstinence
Prolonged abstinence	A measure of cessation which typically allows a 'grace period' following the quit date (usually of about two weeks), to allow for slips/lapses during the first few days when the effect of treatment may still be emerging. See: Hughes 2003
Relapse	A return to regular smoking after a period of abstinence
Secondhand smoke	Also called passive smoking or environmental tobacco smoke (ETS) A mixture of smoke exhaled by smokers and smoke released from smouldering cigarettes, cigars, pipes, bidis, etc. The smoke mixture contains gases and particulates, including nicotine, carcinogens and toxins.
Self-efficacy	The belief that one will be able to change one's behaviour, e.g. to quit smoking
SPC (Summary of Product Characteristics)	Advice from the manufacturers of a drug, agreed with the relevant licensing authority, to enable health professionals to prescribe and use the treatment safely and effectively.
Tapering	A gradual decrease in dose at the end of treatment, as an alternative to abruptly stopping treatment
Tar	The toxic chemicals found in cigarettes. In solid form, it is the brown, tacky residue visible in a cigarette filter and deposited in the lungs of smokers.
Titration	A technique of dosing at low levels at the beginning of treatment, and gradually increasing to full dose over a few days, to allow the body to get used to the drug. It is designed to limit side effects.
Varenicline	A pharmaceutical drug prescribed to treat nicotine addiction; trade names Chantix and Champix

(Continued)

Withdrawal	A variety of behavioural, affective, cognitive and physiological symptoms, usually transient, which occur after use of an addictive drug is reduced or stopped. See: Shiffman 2004

Appendix 4. Quality assessment of economic evaluations

Item	An 2008	Curry 1998	Halpin 2006	Hughes 1991	Joyce 2008	Kaper 2006a	Salize 2009 (Twardella 2007)	Schauffler 2001
1. Is the study population clearly described?	no	no	yes	yes	yes	yes	yes	no
2. Are competing alternatives clearly described?	yes	yes	yes	yes	yes	yes	yes	yes
3. Is a well-defined research question posed in answerable form?	yes	yes	yes	yes	yes	yes	yes	yes
4. Is the economic study design appropriate to the stated objective?	yes	yes	yes	yes	yes	yes	yes	yes
5. Is the chosen time horizon appropriate in order to include relevant costs and consequences?	no	yes	yes	yes	no	yes	yes	yes
6. Is the actual perspective chosen appropriate?	no	yes	yes	yes	no	yes	yes	no
7. Are all important and relevant costs for each alternative identified?	no	no	no	no	no	yes	yes	no
8. Are all costs measured appropriately in physical units?	no	no	no	no	no	yes	no	no
9. Are costs valued appropriately?	no	no	no	no	no	yes	no	no
10. Are all important and relevant outcomes for each alternative identified?	no	no	no	no	no	no	no	no
11. Are all outcomes measured appropriately?	no	no	yes	yes	no	yes	yes	no
12. Are outcomes valued appropriately?	yes	yes	yes	yes	no	yes	yes	yes
13. Is an incremental analysis of costs and outcomes of alternatives performed?	no	no	no	no	yes	yes	yes	no
14. Are all future costs and outcomes discounted appropriately?	yes	yes	yes	yes	no	yes	yes	yes

(Continued)

15. Are all important variables appropriately subjected to sensitivity analysis?	no	no	no	no	no	no	yes	no
16. Do the conclusions follow from the data reported?	yes	yes	yes	no	no	yes	yes	yes
17. Does the study discuss the generalisability of the results to others settings/ patients?	Yes	no	yes	yes	no	no	yes	yes
18. Does the article indicate that there is no potential conflict of interest of researchers and funders?	yes	no	yes	no	yes	yes	yes	no
19. Are ethical and distributional issues discussed appropriately?	no	no	no	no	no	no	no	no
Total score	8	8	12	10	6	15	15	8

WHAT'S NEW

Date	Event	Description
7 March 2017	New search has been performed	Updated for 2017 with 6 new studies
7 March 2017	New citation required but conclusions have not changed	change to authors for update in 2017

HISTORY

Protocol first published: Issue 3, 2003
Review first published: Issue 1, 2005

Date	Event	Description
9 May 2012	New search has been performed	Searches updated; two new included studies and one cost effectiveness analysis of a previously included study added, text updated accordingly.
9 May 2012	New citation required but conclusions have not changed	Change in authors, conclusions largely unchanged.
13 May 2009	Amended	Spelling of author name corrected
3 February 2009	New citation required but conclusions have not changed	Change to authors for update in issue 2, 2009
3 February 2009	New search has been performed	Updated for issue 2, 2009 with 2 new studies
5 November 2008	Amended	Converted to new review format

CONTRIBUTIONS OF AUTHORS

For the current update, FB, GN and AAR selected studies. FB and GN assessed the quality of the included studies and extracted data. DK served as referee in study inclusion. FB conducted the analysis and wrote the update. BW checked the analyses. SE was in charge of study selection, analysis and writing of cost-effectiveness data. All authors critically commented on several drafts of the review. CPS had final responsibility and final check of the text.

DECLARATIONS OF INTEREST

FB: none known
GN: none known
AAR: none known
BW: none known
SE: none known
DK received an unrestricted grant from Pfizer in 2009 for an investigator-initiated trial on the effectiveness of practice nurse counselling and varenicline for smoking cessation in primary care (Dutch Trial Register NTR3067)
CPS and the lead author of the first version of this review conducted one of the trials included in the review (Kaper 2006)

SOURCES OF SUPPORT

Internal sources

- Care and Public Health Research Institute (CAPHRI), Maastricht University, Netherlands.

External sources

- No sources of support supplied

INDEX TERMS

Medical Subject Headings (MeSH)

*Healthcare Financing; *Insurance Coverage; Cost-Benefit Analysis; Financing, Government; Randomized Controlled Trials as Topic; Smoking [*therapy]; Smoking Cessation [economics] [statistics & numerical data]; Tobacco Use Cessation [*economics] [statistics & numerical data]; Tobacco Use Disorder [economics] [*therapy]

MeSH check words

Humans